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THE RELATIVE VALUE OF TIDAL AND UPLAND WATERS IN MAINTAINING RIVERS, ESTUARIES, AND HARBORS.

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Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Mr. Walter R. Browne wished to say, before the discussion commenced, that the current meter used was exhibited on the table; and Mr. Shaw, who had designed it, was present, and would be happy to answer any questions as to its construction, or as to any practical details in carrying out the experiments. Another remark which he desired to make, had reference to a passage he had quoted from Mr. Richardson:—"If any one will look carefully at the different streams in the neighborhood, he will see little streams with little estuaries, medium streams with middle-sized estuaries, and big streams with large estuaries; the channel formed being always in proportion to the amount of fresh water." In a valuable note upon his paper, which had been sent to him from Burmah, Mr. Robert Gordon, M. Inst. C. E., while in a great measure assenting to Mr. Browne's conclusions, took exception to that passage, and said it was contrary to his own experience, and to the facts with regard to the rivers in the East, and also on the west coasts of Scotland and Norway. He would not go into the question of the rivers of the East, with which he was not acquainted; but he was desirous of pointing out that Mr. Richardson's remarks and his deductions from them, only applied to rivers that ran into the sea through flat alluvial lands—the only cases where there was much mud, and where the question of tidal scour came into prominence. In such cases as those of the glens of Scotland and the fiords of Norway, the outline of an estuary was simply the contour line of a particular level in the old valley, which valley had been scooped out by ice, or by subaerial waste, or by whatever agent geologists might finally agree upon as that by which valleys were excavated—that level being what happened to be high-water level at the present day, and having no relation to any question of scour. In fact, in all such cases the waters were perfectly clear both outside and inside the fiord, and there was practically no scour at all.

Mr. W. Shelford was under the impression that the paper was founded upon erroneous premises and imperfect experiments, and, as the conclusion was condemnatory of the whole modern school of engineering, it was revolutionary. The author's first line of argument was that the silt which tended to choke up tidal channels was almost wholly due to tidal water, and not to the fresh water; and many others of that opinion; but he

would state why he differed from them. It would be admitted that such rivers as discharged into tideless seas, like the Nile and the Tiber, delivered whatever material was in the waters into the seas, and formed deltas, such deltas having existed for centuries, that of the Nile being 100 miles wide, and that of the Danube 50 miles wide. The author had referred to the Parret, and stated that the mud near Bridgewater, from which Bath bricks were made, came, not from the watershed of the Parret, but from a cliff of sandy clay below. Mr. Shelford did not know the Parret sufficiently to be able to contest that point, but he was well acquainted with the Humber, and was aware that it was one of the best tidal rivers, discharging the largest amount of fresh water of any river in England, and also being the most muddy river in England. For years the origin of the mud had perplexed many minds. It had been argued that it came from the degradation of the sea coast between Spurn Point and Flamborough Head, a length of 30 miles, where the wasting of the cliff was very great, averaging 2¼ yards in width per annum. His own belief was that the mud came from the watershed of the Humber, which was 10,000 square miles, and that the rain falling upon that watershed brought down with it a quantity of deposit, depending upon the volume of fresh water, into the estuary, and it could go nowhere else. The coast degraded by the sea was 30 miles long by ¼ mile wide, say, 7½ square miles as against 10,000. Besides, all the material derived from the cliff could not enter the Humber, because it was first scattered about the sea, and a great part of the water passing the cliff never entered the Humber at all. Then it should not be forgotten that the rate of transport of silt, or of matters in suspension in a tidal channel, was much less than that of a fresh-water channel into a tideless sea. The transport of such rivers as the Nile and the Danube, which contained large quantities of silt, would be found to be at least a hundred times that of the transport of a tidal river. He founded his observation upon experiments made by Mr. Bateman, Past-Pres. Inst. C. E., of the progress of sewage in the Clyde, which he believed was at the rate of 5 miles in a fortnight; and that had been confirmed by observations subsequently taken in the Thames. The material which came down from rivers discharging into a tideless sea was, in a tidal estuary, carried backwards and forwards, and could find no rest. At the mouth of the Humber and of the Thames there was no delta, while at the mouth of rivers discharging into tideless seas there were large deltas; and the obvious conclusion was that the material, for the most part, traveled backwards and forwards. Some of it, no doubt, was deposited on the sides; that was the case with the Thames and the Humber, where the whole width of the estuaries had been narrowed considerably within historical periods, but the tidal action was better than ever. To prove that it was tidal action, he might mention that the low water at London Bridge was lower than low water at sea. That could only happen by the action of the tide, and could not take place if it were a low-water river discharging into a motionless body of water, such as the author has described. The second argument of the author was that, on the whole, the tidal water tended to choke the channel, and not to scour it. Before that could be established, some awkward facts would have to be got rid of. The Tiber and the Thames were fairly comparable rivers above Rome and London. At its outfall the Tiber was a river whose function was simply to discharge the fresh water that came down past Rome. Its channel was not passable for a boat; it was only 18 inches in depth, and it was encumbered by shoals thrown up by storms at sea. The Thames, on the other hand, was a magnificent river. The author did not appear to be satisfied with his experiments on the first day, and therefore, they were repeated six months later—he supposed with the little instrument exhibited—and found some results which were certainly most interesting, if true. They were surprising even to the author, but he had not repeated them; and Mr. Shelford ventured to think, knowing as he did the difficulty of making such experiments with accuracy, that unless they were repeated frequently, there was nothing in them to justify the conclusion which the author had drawn.

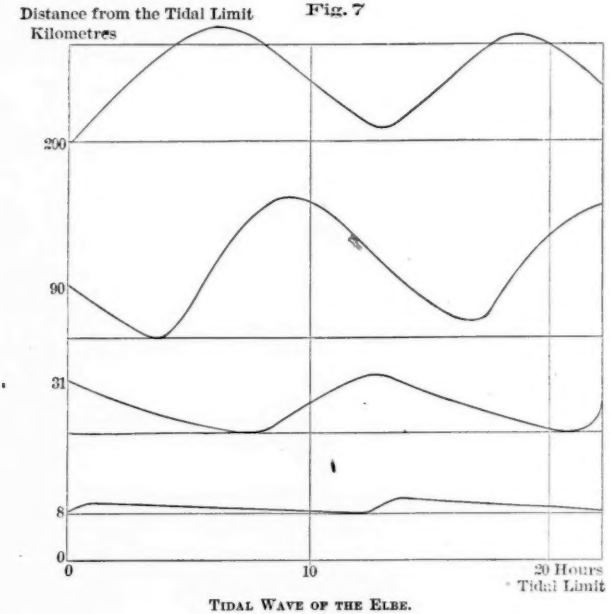
Prof. W. C. Unwin thought it might be interesting to place alongside the meter used for the author's experiments, one or two meters in his possession. One of

them, received from France, was a double Pitot tube, and had been employed by Darcy and Bazin in the most celebrated series of researches ever made on the flow of water in channels; it was, however, only applicable in channels of moderate depth. He had also one of the latest forms of German current meters, which might compare with that adopted by the author. It could be used in the same way as the ordinary current meter. It had a screw fan and a counter; it could be put on a staff, and had a rudder for directing it in the direction of the current. There was, however, one difference from the ordinary meter. The great weakness of the English form of meter was the disengaging arrangement for putting the counter into and out of gear. The meter to which he had referred, had a double ratchet arrangement. Alternate pulls, of whatever strength, put the counter into gear and out of gear. A second defect in the English form was that it had to be taken out of the water for every observation, which involved a great waste of time. In the German meter, that difficulty was got over by an arrangement, which might be used or not, of an electric bell, which sounded at every hundred revolutions. The meter could be sent down in the ordinary way, connected with the battery, and while it was under water the bell sounded at every hundred revolutions. It was only necessary to have a stop watch, and to note the time, say, at every two hundred or five hundred revolutions, and the observations could be repeated as often as was required without moving the meter. A third defect in the ordinary meter was that it could only be used on a staff, and that limited its use to comparatively moderate depths; at all events, its use in great depths was extremely inconvenient. The German meter could be fixed on a universal joint and suspended from a wire. It had a peculiar form of rudder; it was perfectly balanced in the water, and it had below a large weight of 60 lbs. There was a rather ingenious crane by which it could be lowered, and which had a further function of registering the depth of the meter. In addition there was a contrivance for using the meter for sounding, an electric signal noting the arrival of the weight at bottom. Professor Harlacher, who had made more experiments on the screw meter than anyone else, had effected some further improvements in the instrument. He objected to the free swinging of the meter, and he had shown some methods of getting over that objection in a work to which he need not further allude.

On reading the paper it seemed to him as though the author thought it desirable that accepted theories should be occasionally attacked, and that it was well to be forced to look carefully into the grounds on which the opinions of engineers were founded. But there were assertions in the paper which could hardly be supported. When he found it stated that all channels which had no upland waters completely silted up; that it was a well-known fact that all ordinary rivers kept a constant regimen; that silt was deposited most thickly in the deepest part of the river, in spite of the fact that in the deepest part of a river the velocity was greatest; when he was assured that in very deep rivers the bottom velocity was the same as the top velocity; and when, in support of that statement, there were quoted the experiments of Humphreys and Abbot on the Mississippi, although those authors believed that they had established the fact that the vertical velocity curve in a river was a parabola with a horizontal axis at one-third the depth from the surface, so that the bottom velocity must be less than the top; when he was told that for maintaining the channel the entrance of tidal water could be nothing but an evil; he was forced to conclude that the author had been somewhat rash in his assertions. There was a mathematical calculation, in which the square of the velocity of the river was made proportional not to the slope of the river but to the depth of the river. He could not help thinking that the author had been pushing to an extreme his argument for a cause which was not a very strong one. The title of the paper was, "On the Relative Value of Tidal and Upland Waters," but throughout the whole paper no mention was made, except in one paragraph, of upland waters. The author had dealt with what he called the low-water flow of the river. Now, the low-water flow and the upland waters were totally different. It was desirable to understand exactly what the low-water flow of a river was. For this purpose reference might be made to Mr.

D. Stevenson's book,* where a diagram was given of the tidal wave at two points 9 miles apart, in the Dornoch Firth in Scotland, the first point being where the sea wave was almost unaffected by the obstructions of the river, and the second where the obstructions had shortened the time of the flow and increased the time of the ebb. The diagram showed a perfectly flowing curve from the top of high water, and there was no time at which the low-water flow did not contain a large proportion of tidal water.† During the last month or two he had become acquainted with some researches on the Elbe, and he had drawn from a paper of Herr Löhmann four tidal waves observed in that river (Fig. 7); the first was eight kilometers below the point where the tidal influence absolutely ceased, the next at 31 kilometers, the next at 90, and the next at 200. In all those curves the low-water flow consisted chiefly of tidal water. But engineers were not restricted to the use of current meter observations in determining the exact quantity of tidal water at the different sections of a river, and Herr Löhmann had carried out a remarkable investigation of the flow of water in different sections of the Elbe without any use of the current meter. At a series of thirty or forty stations along the length of the river, there were observers, noting at the end of each hour the height of the river, and thus curves of the water surface were obtained for each hour up to the point where the tidal influence ceased. Suppose two of these curves plotted, say, for twelve o'clock and one o'clock, it was obvious that past any section there had flowed into or out of the river during that hour a volume of water equal to the space between the river surface curves above the section + the upland water if the river was falling, − the upland water, if the river was rising. Herr Löhmann had drawn the curve of the water surface of the river for each hour of one tide, and from that he had calculated the quantity of water flowing past different sections at each hour. The results were plotted in Fig. 8. The horizontal line represented 140 kilometers. The minute space between the

Fig. 7

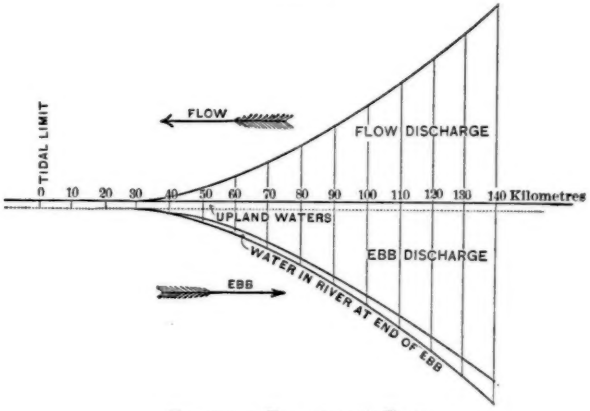


TIDAL WAVE OF THE ELBE.

* *Vide* "The Principles and Practice of Canal and River Engineering," 2d edition, 1872, p. 62.

† This was perhaps stated too absolutely. It would not be true of portions of the river near the limit of the tidal action.—W. C. U.

FIG. 8.


TIDAL LIMIT
FLOW
FLOW DISCHARGE
0 10 20 30 40 50 60 70 80 90 100 110 120 130 140 Kilometres
UPLAND WATERS
EBB DISCHARGE
WATER IN RIVER AT END OF EBB
EBB

VOLUMES OF FLOW FOR ONE TIDE.

black line and the dotted line represented the quantity of upland water flowing down the river. The quantity of tidal water passing each section was represented by the ordinate of the upper curve for the flood, and of the lower curve for the ebb. Between the two small curves at the bottom was the quantity of water remaining in the river at the end of the ebb. Comparing the enormous quantity of the flow at almost all sections of the river during the flood and during the ebb with the minute quantity of upland water, it would be tolerably obvious that throughout the greater portion of the tidal part of the river the tidal water must have a much greater influence than the upland water. But Herr Löhmann was not quite satisfied with the curves, and he had tested the thing in another way. He had calculated the quantity of water flowing in one hour for each meter breadth of the river, in percentage of the upland water. As the river approached its mouth it got wider, and with upland water alone so wide a channel would not be maintained; still it might be useful to find what the quantity of the flow was for each part of the river, and Herr Löhmann had given the necessary data.

That seemed to show that not only the total flow of water, but the scouring action per square foot of bottom, steadily and largely increased from the point where the tide ceased downwards to the mouth of the river. To this the author raised the objection that the silt in the river was brought up by the flood-water, and he referred to the fact, that while river water was comparatively clean, the tidal water going up and down was very muddy. The greater muddiness of the water in the estuary, compared with the water in the river above, merely proved that the tidal water had very large scouring power—it had scoured the bottom of the river much more, and had become much more muddy. It still, therefore, remained a question, what became of the muddiness of the water in the tidal compartment of the river? There was no doubt that the tidal water coming into the river was clean sea water, and the water going out into the sea was no longer clean sea water; it was not only muddier than the river water, but it was very much greater in volume; therefore, the muddiness in the estuary was, he thought, mainly carried out to sea. But the question was one which required a different investigation from any then before the members. The author had quoted a number of instances where, on the shallow shores of estuaries, and in the case of the Parret on

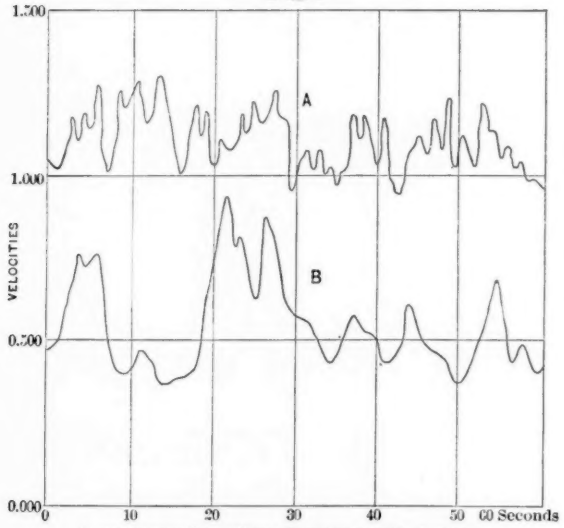
MEAN HOURLY FLOW PER METER WIDTH OF RIVER AT DIFFERENT SECTIONS, IN FRACTIONS OF UPLAND WATER.

Distance from point where tide ceased	—	50	80	110	Kilos. 140
Ebb.....	1.0	1.06	2.41	3.57	3.82 "
Flow.....	—	1.10	2.59	3.97	4.40 "

specially prepared shelves, there was a deposit of silt. But he had ignored the fact, that in one section of the river there might be going on at the same time a scour in the middle part of the channel and a silting up on the sides. He could not find any case mentioned in the paper which might not be explained in that way.

There were one or two points with reference to the experiments to which he should like to allude. There was, he thought, throughout the paper a misconception about the action of the water in a river—that at a given point there was a definite velocity; for example, at 6 feet below the surface and at 20 feet from the bank. He believed that was an absolute delusion. The motion of the water was, for theoretical purposes, often treated as if it were motion in plane layers at a steady velocity; but the actual motion of the water was immensely more complicated. The amount of viscous resistance of water was absolutely known, and it had been clearly shown that, if the water moved in simple plane layers, rivers would have some hundreds of times their actual velocity. The fact was, that in a river an enormous amount of work was wasted, and there was an enormous amount of retardation, from eddying motions superposed on the general motion of translation of the water. Prof. Harlacher had tested that in a curious way. He had connected one of the electric current meters with a very delicate recording instrument, like Morse's telegraphic recording strip, and had thus recorded every revolution of the meter. The result was shown roughly in Fig. 9, and it would be seen, from one observation near the surface and one near the bottom, how enormously the velocity varied during a period of one minute. One thing was certain, that the inertia of the meter reduced the fluctuations of the curve. The meter was more or less heavy; and if it were perfectly weightless the curves would vary much more than they did in the diagram. The author stated that it was an obvious fact that the scouring action of any current was solely due to the layers of water in proximity to the sides and bottom. But that statement seemed in two ways in error. The scour was due to the eddying agitation of the water, which did not necessarily depend on the mean local velocity at the bottom of the river. In the next place, suppose the silt to be scoured up by the water and mixed with a body of water, it was then carried down stream, again deposited somewhere, again scoured

Fig. 9



A, meter near the surface; B, meter near the bottom.

up and deposited, till it got out to sea. The rate of transport of the silt down the river depended, not on the bottom velocity, but on the mean velocity, and it would be more rapid in a river which had a high mean velocity, than in a river which had a low mean velocity. The rate of transport down the river should be considered, as well as the rate at which the silt was picked up from the bottom. He thought the author had not been fortunate in his selection of results as to the vertical velocity curve. He had sought to prove that there was a certain ratio between surface and bottom velocity, and for that purpose had compared a number of experiments. He did not like to deal with a question of that sort without first considering it in the author's way, and he had worked out some of the results of Harlacher's observations, which had been carried out with more care than any previous experiments. Harlacher had been testing some sections of the Danube where the depth reached 8 meters, and some sections of the Elbe where the depth was only 2 meters, and he had given the proportion of bottom to surface velocity, which was greater in the deep stream and less in the shallow stream. He did not place much reliance upon the result, but it showed that by taking a small number of experiments, almost any result might be obtained. The author seemed to place some reliance upon the results obtained by M. Révy in South America. They were made, no doubt with extreme care and under exceptionally difficult circumstances, and they resulted in an altogether extraordinary law as to the vertical velocity curve in a river. Révy found that the velocity varied inversely as the depth; but that was contradicted by every other experiment, and it could not be received as a general law.* The author in summarizing the results, had arrived at a conclusion that in very large rivers, the bottom velocity was practically the same as the surface velocity. Not only was this mechanically impossible, but to obtain this law he had discarded M. Révy's result, which he had previously quoted, and he relied on the Mississippi results, which were altogether different; lastly, he relied upon some experiments by Mr. Gordon, on the Irrawady. Those who knew what had been doing in hydraulics would be aware that there was something peculiar about those experiments. Seven thousand series of observations had been sent by Mr. Gordon to the great French hydraulician, M. Bazin, and the only way in which M. Bazin could deal with them was by getting a series of computers to work at them, so as to average them, and then the singular result was arrived at that, as a river rose in the flood, the bottom velocity got higher and higher, and that at last the bottom was greater than the top velocity. He believed the experiments were made with the greatest care, and with enthusiasm; but he thought with M. Bazin, that there were some local conditions which altered the result, or that the method of experiment was one which did not admit of accuracy. It was curious that the two sets of experiments which had given trouble in regard to the theory of rivers were the Mississippi experiments and Mr. Gordon's. Both of them had been made by a peculiar method of using double floats.

The scouring action in a river was attributed by the author to direct action of the low-water stream, and thence the conclusion was drawn that the entrance of tidal water into a river was nothing but an evil. Now there was a way of admitting that the upland waters assisted the tidal waters, which did not involve such a conclusion. No doubt the upland waters did act powerfully in maintaining the uppermost section of the tidal compartment of the river, the only part of the river where the tidal influence was likely to be prejudicial. By maintaining this portion of the channel, part of the tidal reservoir was conserved, and thus indirectly assisted in maintaining the river lower down. But if this was the way in which upland water acted, it still remained a canon of practice in river improvement to facilitate in every way the volume and rapidity of the tidal flow.

Mr. J. B. Redman said that, whatever opinion might be formed as to the deductions drawn by the author, all must acknowledge an indebtedness to him in bringing forward for the first time an ex-

* Probably Bazin's formula was the best expression for the relation of the maximum and bottom velocity

$$v_b = V - 36.3\sqrt{hi}$$

where, v_b = bottom velocity; V = maximum velocity; h = depth, and i = slope. But obviously it depended on the roughness of the bottom and other considerations, which could not at present be taken into account.—W. C. U.

ceedingly important subject. Accepting the dictum of the author, that the scour of a great navigable tidal river was due to the upland water, and also the second proposition that the silt was brought up from the sea; taking the case of the Thames, where the upland water, in extreme flood, was one-twentieth of the tidal water, that river, in the course of time, must cease to exist. But the author might take heart, inasmuch as the Astronomer Royal, Sir George Airy, in 1877, advanced a theory that, as the ebb lasted seven hours, and the flood five, the transporting power of the flood was as 7 to 5. Accepting the Astronomer Royal's dictum, the Thames, in the same manner, must cease to exist. He accepted as truthful the author's assumption that the silt in a river like the Thames came from the sea, and he thought that in the case of the metropolitan river, the least observant would see that that must be so. During a neap tide, especially during the last neap tides, the river water within the metropolitan boundary was singularly clear. If the remark were made to a man working on the river, he would say, "Yes, but wait till the spring tides, and you will find that the river is muddy enough." Along the length of the river, from Blackwall to Sheerness on the north, and from Greenwich to the Isle of Grain on the south, might be observed the "saltings," which denoted the range of neap tides, and it would be seen that they were serrated and broken down by the spring tides. The spring tides was, of course, of much greater volume and much higher in altitude in its flow, and it stirred up the bed and muddy foreshores of the river. A large amount of deposit from the spring tide was brought into the upper part of the river, but inasmuch as the ebb was seven hours as compared with five of flood, the ebb removed a large amount of deposit by progressive steps down the river. He thought a somewhat indiscriminate statement had been made, when three rivers were mentioned to prove that the author's supposition was wrong, that silt was carried up the river, namely, the Tiber, the Danube, and the Nile—three tideless rivers, falling into tideless seas. There could not be any analogy between those rivers and tidal rivers. In reference to the large amount of deposit in the river coming from the sea, a few years back he was engaged in an inquiry as to an accumulation of mud and sand in the Gravesend reach of the river. It was alleged that the accumulation arose from the erection of the Terrace pier. On inquiry he found below the town that there was an old military landing place, called the "Ordnance" jetty, used for embarking and disembarking troops; there was also another bridge, called the New Tavern bridge, which, at that time, was silted up to the planks. On pursuing the inquiry, he found the Custom House bridge had succeeded the New Tavern bridge, and that the New Tavern bridge stood upon one that preceded it. The accumulation had gone on since the close of the last century, and amounted to about 3½ inches per annum. It was entirely local in the hollow of the river, and was not only promoted by these bridges, but was also kept by them in position. Higher up the river at Northfleet the shore was cleaner, and at Greenhithe, in front of "Ingress Abbey," the owner, an old sailor, had often remarked to him that the foreshore in front of his estate was frequently temporarily covered with a large deposit of mud. He had himself noticed after a gale of wind blowing up the reach, and impinging on the shore, that the foreshore was furrowed almost like a ploughed field down to the virgin clay, and the mud disappeared; but on a recurrence of calm weather the accumulation again formed. There was a constant deposit from the flood tide, which was dispersed under certain conditions.

He wished to refer to that portion of the paper which spoke of a tidal harbor being kept open more by the upland water than by the tidal water; and he would inquire how it came to pass that such harbors as Harwich, Portsmouth, Yarmouth, and Montrose, had maintained their depths for centuries. Portsmouth had 13½ million tons of tidal water, with a width of entrance of 500 to 600 feet, and a minimum low-water depth of 17½ to 20 feet. Harwich had 60 million tons of tidal water, a width of ¾ mile, and a minimum navigable depth of 17 feet. Yarmouth had 6 million tons of tidal water, a width of 200 to 300 feet, and a navigable depth (since the time of Elizabeth) of 10 feet. Montrose, with 40 or 50 million of tons of tidal water, and a width of a furlong, had a navigable depth of from 8

to 10 feet. There were two rivers flowing into Harwich, two small streams into Portsmouth, and three into Yarmouth; but the amount of upland water was infinitesimal.*

On the authority of Mr. Richardson, it was stated that the estuaries of tidal rivers always bore a certain proportion to the upland water falling into the catchment basin. He had had the curiosity to look at the catchment basins of several of the principal rivers in England. Taking the Thames and the Humber, two rivers fulfilling the conditions mentioned by the author, as flowing through alluvial soil, the catchment area of the Thames was 5,000 square miles; the width at high water was 5½ miles, and at low water 3 miles, and the depth from 30 to 60 feet. The Humber had a catchment area of 9,480 square miles, the width at high water being 4 to 5 miles, and at low water 3 miles, and the depth 30 to 70 feet. The catchment area of the Great Wash, into which the Great Ouse, the Nene, the Witham, and the Welland fell, was intermediate between that of the Thames and the Humber, viz., 5,734 square miles; the width was double, 10 miles, and the depth was very great, 30 to 70 feet. The Great Wash was the channel for the influx and efflux of a vast quantity of tidal water before the works carried out by Sir Charles Vermuyden and subsequent engineers were completed, and when a large tract of fen-land was under water; and the ten miles of estuary represented the power of tidal water to keep open the channel. Before concluding, he desired to direct attention to a few words uttered by the late Mr. G. P. Bidder, Past-Pres. Inst. C. E., in an inaugural address. "Points of the utmost importance, upon which to arrive at a clear understanding, are the relative effects of the scour upon our rivers and harbors, arising from the action of tidal, or of land waters. I am free to admit that I am one of those who attribute the smallest possible effect to the action of land waters, whilst I do attribute the most important effects to the action of tidal scour. This conviction has been forced upon me by a general view of our rivers and estuaries. Looking, for instance, to the river flowing past our doors, the river Thames, can it be supposed that the mere driblet of water falling over Teddington Lock, even in seasons of the highest floods, can give any effectual aid to the tidal currents, in preserving the channel from sea to the Nore, and up to above Gravesend." Notwithstanding, he felt sure the author of the paper, possessing comparative youth, acknowledged talent, and, no doubt, courage worthy of the name he bore, would make an excellent fight to maintain the position he had put forward on that great question.

Mr. J. Thornhill Harrison remarked that the paper opened out so many questions, such as the sources of the silt and sand, whether they were brought from above or below; the measures that engineers had adopted to restrain them in both directions; the improvement of rivers, and the means of scouring out falls, that he would merely refer to them, and would go at once to a point which he thought was of great importance. The author had laid down certain rules and principles which deserved serious consideration. Upon what did the author base those rules and principles? So far as could be judged, entirely upon observations made by Mr. Shaw at one spot on the river Avon. But were those experiments sufficient, and were the deductions from the experiments correct? It would be noticed that the point where the observations were taken was 7½ miles from the mouth, and only two miles from the upper tidal limit, whereas they ought to have been made nearer the mouth. Again, the experiments were of a very partial character, were only continued over about three hours of tidal action, and were not taken at any other place upon the river; so that there was no information as to the velocity of the water at any other spot, either at the surface or below it, nor as to the volume of water flowing from the river proper above; nor were there any data as to the fall of the water from the point where the observations were taken to the mouth of the river. He thought that, before rules and principles were laid down, experiments should be made not only at one spot upon

* It must at the same time be allowed that of late years dredging operations had maintained and improved the navigable low-water depths of those harbors, and doubtless this would be increasingly so in future years, from the continued increase in the size of steam vessels in the mercantile marine, and the great improvements developed in dredging plant, and reduced cost of raising and depositing the material, greatly resultant on such works as the Suez and Amsterdam ship canals.—J. B. R.

one river, but at many spots throughout the whole tidal range upon many rivers. The information, however, so far as it extended, supplied some useful hints. He had prepared some diagrams from the particulars given by the author, which showed that, for a considerable time from the commencement of the ebb, the water fell and the velocity increased almost exactly in proportion as a body would fall freely under the action of gravity. He had marked three points, the first period up to 9.55 A.M., when there was some check to the increase of the velocity, and a check also to the rate of fall of the water. After a short time both of them became regular; the fall of the water went on regularly, so much every hour, and the velocity became constant. That period he had marked as the second. Then again there was a third period when the velocity diminished, and when the rate of fall of the water also diminished; that went on to the point where apparently the low-water level was reached, or nearly so; the experiments ought to have been continued not only to low water but beyond it. The diagrams appeared to show that the river Avon at high tide was full of water up to Netham dam, the surface having a very slight fall from the dam down to Avonmouth, and the water had, during the ebb, to fall from its elevated position down to low water. The water could only fall vertically at the Netham dam, there could be no lateral motion; but the vertical motion as the water fell was transmitted horizontally, creating a wave motion, which was passed onwards down the river, and taking any point, such as that at which the observations were made, the quantity of water that would pass that point in a given time must be equal to the quantity that had fallen in that time between Netham dam and the point of observation. It was proved that the whole of the water from the top to the bottom was not put in motion, but only the upper portion to an unascertained depth. The volume of water in motion must increase gradually from Netham dam downwards, and, supposing the velocity of the current to be nearly the same throughout the course of the river, the water in motion would take the form of a wedge, whilst the water below this wedge would remain at rest; and so long as there was sufficient depth of water for the generation of the wave, the conversion of the vertical fall into a horizontal motion would be quite free, the motion between the particles of water being almost free from friction. Mr. Shaw's experiments showed that for a period of an hour and a quarter after the tide began to ebb, there was at the point of observation, a steady generation of a wave of translation, as the velocity increased with the time, and the fall of the water increased as the square of the time. But there would come a time when the depth of water in the channel would be so far diminished, and the quantity of water discharged from the river would be so great, that the water would be in motion from the top to the bottom of the channel at some certain point, and then the free generation of the wave would be arrested. That appeared to be the commencement of what he had marked as the second period when, the free fall of the water being stayed, and the velocity not being increased, there was a steady fall directly as the time, the velocity of the water at the moment of the observation continuing constant. The commencement of the third period appeared to be when the tide had so far fallen that the water was in motion from the top to the bottom. That the ebb tide created a wave in the way described was, he thought, proved satisfactorily by the fact that the low-water level at Glasgow and at London Bridge was frequently lower than it was some miles below Glasgow in the one case, and Sheerness in the other. This showed that there was a pendulous action, and that the momentum given to the water towards the close of the ebb tide was much greater than it would be if it were due merely to the inclination of the surface at low water—the water was in fact forced forwards and carried onwards to a lower level even than the low water at the sea. When there was a weir, like the Netham dam or the Teddington weir, which arrested the passage of the tidal water upwards, the water adjoining that weir must necessarily be in a state of stagnation, from the time the tide reached it until it rose to its highest level and fell again and passed onwards. There could be no motion at that point, except that given by the flow of the water over the weir. As far as tidal action went, if there were no

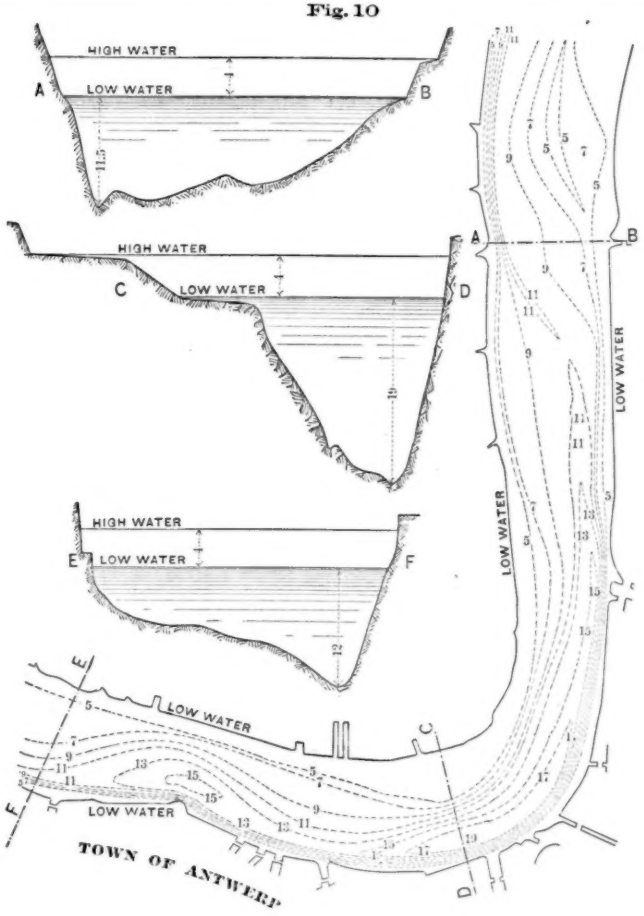
water flowing over the weir the water would be stagnant adjoining it; it would fall downwards, but would have no lateral motion; consequently during that whole period, as the author had pointed out, there would necessarily be a deposit, and this deposit would diminish downwards. To what did that point? That every weir of the kind should be cleared away, and that tidal action should have a free flow as far up the river as possible; it would then pass up the river proper with a rising bed, and the quantity of deposit in the upper portion would be reduced to a minimum. Mr. Shaw's observations did not prove either that the bottom velocity, not far from the mouth of a tidal channel, was *nil* for a great part of the ebb, or that the scouring effect of the ebb tide was little or nothing.

Mr. H. Law had been much startled by the novel views advanced by the author, and the insufficient data upon which, he thought, they were founded. The opening sentence of the paper he could not comprehend. The flow was divided into two parts. There was the tidal flow, which occupied twelve hours in the ebb and flood together; and then the low-water flow, which was strictly defined to be the time which elapsed between the end of the ebb and the beginning of the flood. Now, inasmuch as the ebb designated the water flowing downwards and the flood the water flowing upwards, he was at a loss to conceive what could be meant by the motion of the water at the end of the ebb and before the flood began. Clearly the water must then be stagnant; and it was impossible to maintain, even if there were any short interval, that what was defined to be the low-water flow could produce the whole of the scouring effect in a river, and not the tidal flow, which occupied a space of time much longer, the mean flow over Teddington weir being about one-three-hundred-and-fortieth of the mean flow of the tide. Then followed this clear and precise statement: "In the author's opinion, this assumption [that the tidal flow produced the scour] cannot be maintained, and grounds quite as solid might even be found for exactly the opposite rule, viz., that tidal water should be wholly excluded from a channel, wherever possible." He thought that was a proposition which entirely controverted all the established principles and experience of the profession, and which certainly required a more extensive series of observations than had been adduced in the paper. The instrument which had been used in the experiments had been laid before the Institution. Mr. Law's experience with current meters had been considerable, and he had found after a short time that attempting to take observations in deep water with rapid currents by the old system of withdrawing a detent, which allowed the instrument to revolve and then allowing the detent to fall into gear again, was quite impracticable. If there was any stream and any length of rope out the tension upon the rope alone was sufficient to withdraw the detent and set the machine in motion, and it was impossible to tell whether it was running or not. Then there was the great practical difficulty of having to bring the instrument to the surface to make every observation; and the time occupied to bring it to the surface and lower it again prevented that rapid succession of observations which alone would enable the observer to determine nice differences in the velocity of the current at different depths, and at different periods of the tide. It was that experience, after having tried all the meters he could find, that induced him at last to contrive the machine now exhibited, by which the current could be measured by means of electricity at any depth and continuously. He had found it was necessary, in order to give steadiness to the machine, to have a very heavy bar. Mr. Révy employed one of about 30 lbs.; but Mr. Law had adopted one weighing 90 lbs. The most desirable way of lowering it was by attaching it at each end, and that was done by angle-irons projecting over the sides of the boat at right angles, and the bar was lowered in a perfectly parallel position. The meter was attached in such a manner that it could present itself to the current. Wherever a strong wind was blowing or there was a current on the surface in a different direction, it was necessary that the meter should assume its own position below. He had tried many expedients for insulating the current, and at last that which suggested itself to him, and which was the most simple of all, was that of

placing the insulating apparatus at the top of a vertical spindle, and enclosing the whole in a small diving bell, the air in the upper part of which served to prevent the water ever reaching the insulating apparatus. There being no stuffing box, the meter revolved with perfect freedom. In the air when exposed to a moderate breeze it made six hundred revolutions a minute by virtue of the wind alone. The ball in the center of the screw was hollow and balanced the weight, as in Révy's instrument, and the consequence was it possessed that quality which Professor Unwin said was necessary, a very small *vis viva*, so that it could rapidly change its motion with the varying motion of the currents. At every hundred revolutions the bell was struck and the signal given above. In order to determine the depth there were two cylinders, with two cables wound round them. As the cable might vary in size according to the material used, the cylinders were split in two, and by a simple mechanical arrangement could be adjusted, so that one circumference gave exactly 18 inches of cable. There was then a scale upon the instrument; a guide traveled upon a screw, which at the same time ensured an even winding of the cable, and the depth could be seen by inspection. The number of seconds that elapsed in every hundred revolutions gave the velocity of the current. The instrument was also adapted for sounding. It was quite as necessary to have an accurate section of the river as it was to have the velocity of the stream. For the purpose of sounding, the two angle-irons were brought together over the boat's side, and the two ends of the wire were connected with a sounder which weighed 50 lbs. The moment it touched the ground it struck the bell, and the depth was exhibited upon a scale. It could be then raised a foot or two, and passed to another point, and by getting the signal the depth was obtained immediately. The sounder weighing 50 lbs., and the line being so small, however great the velocity, no deflection took place and it hung vertically. The instrument exhibited was the one with which he had tried a great many experiments, and he must say that the results he had obtained were very different from those arrived at by the author.

Mr. Russel Aitken thought the value to be attached to the author's tidal observations was being lost sight of. The range of tide was about 36 feet, and it was evident that, as the tide rose, water would be found in the bottom of the channel of a greater specific gravity than at the top. A layer of cold water or salter water would probably be at the bottom. The top water would slide on the top of the heavier water, and so soon as the tide got far enough down, and the heavier fell below the meter, it began to turn. It was well known that layers of water having different specific gravities slid upon one another in the most wonderful manner. The Gulf Stream, although it was salter, yet having a higher temperature, was lighter than the waters of the ocean through which it flowed, and slid through the ocean, and so he believed it was in the case under consideration. It would be found that the author's observations could be accounted for in the way he had mentioned. He thought Professor Unwin should not consider water as working, as a general rule, in layers. He believed that when water in a river was of the same specific gravity, and this was generally the case as water in a river got well mixed up, the water above pushed the water below solidly along, and the retarding effect of the eddies caused by friction on the bottom did not cause water to go in layers, but gave it a rolling motion. Until recent experiments the theory that water flowed in layers was generally adopted and acted upon, but this was evidently incorrect. He had represented on a diagram the river Scheldt (Fig. 10) which was full of silt, and was interesting as showing the value of the land water at low water, in extending the low-water channel. The Scheldt went past Antwerp from D to E. It would be observed that at D there was a depth of 19 meters, and the rise of the tide was 4 meters. At AB a little further down there was a depth of 11.5 meters, and further up there was a depth of 11 or 12 meters. At AB the width was the same as that of the Thames at Woolwich, and the tide only rose 4 meters, or 13 feet, as against 18 feet 6 inches in the Thames; yet there was a navigable channel of 26 feet or 28 feet, or not less than 8 meters at low water, whereas in the Thames there was

Fig. 10



only a navigable channel of 14 feet. Of course, in the Thames the bottom was sand and gravel to some extent, whereas in the Scheldt it was fine sand. He thought that a great deal of the depth observed in the Scheldt was due to the exact time at which the water issued from the dykes. The land around the river was situated at a very low level, rather below half tide. The sluices opened a little before low water, and then there was a great current due to the land waters. The river Scheldt was a remarkable instance of how the scour in rivers was assisted by the introduction of land waters at the moment when they could be most effective. There was a general opinion, which he believed to be erroneous, that when water flowed in a river it rebounded from one side to the other. Water, like every other kind of matter in motion, will proceed in a straight line until it met an obstacle, and when it met a bend in a channel it heaped up against the side towards which it went. Therefore at the point below D it would be found that the water was heaped up, and was actually pressing the water further down, and forcing it towards the other side of the river. In every other river, the water changed from side to side, owing, he believed, to the water heaped up on one side pressing

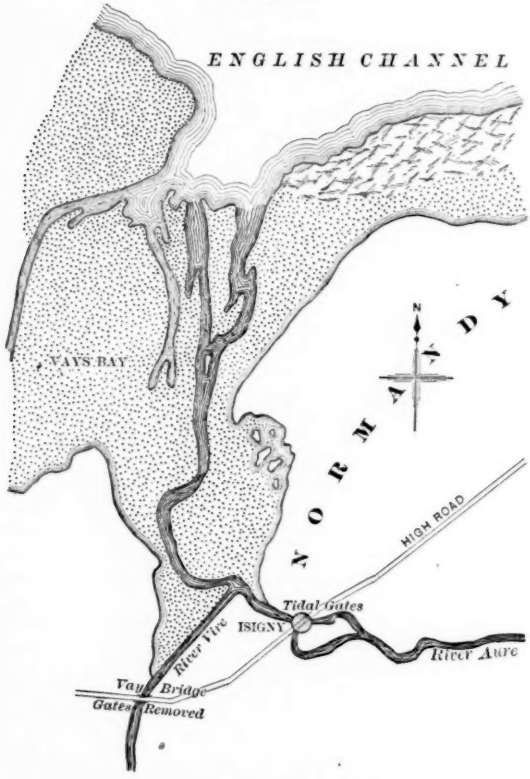
the water back to the other, and it was probably this heaping up of water at the bends which caused the difference of velocities in the various depths of the river, which had been observed by Messrs. Humphreys and Abbot. Professor Unwin had referred to a number of experiments which had been made on small rivers, and had cast a doubt on Mr. Gordon's observations on the Ganges and the Irrawaddy. Mr. Gordon believed that the scour in a river was greatest on the rise of the flood in a fresh-water river. Mr. Aitken thought that statement was correct, and if analyzed it would be seen that it must be so. Take the case of a large river with a gradual fall of, say, 3½ inches per mile, and flowing at the rate of about 5 miles an hour, if the river in flood rose at the rate of 5 inches per hour, then the fall of the river evidently increased by 1 inch per mile during the rise of the flood. In other words, the fall, instead of being 3½ inches, was 4½ inches per mile; therefore there was an increased fall, giving a greater velocity which would of course augment the scouring power of the water. When Mr. Bradford Leslie, M. Inst. C. E., was building the Gorai bridge, over a branch of the Ganges, he had expressed the opinion that the greatest scour took place on the rise of a flood in the river. A cylinder 14 feet diameter and 30 feet long, had been lost on the rise of a flood. It seemed that scour took place with the greater velocity of the rise, and that deposit again occurred during the diminished velocity of the falling flood in the river. If, in connection with the paper, the temperature and the density of the water at different levels had been given, it might have been seen better what was going on. Until that information was supplied, he could not account for the sudden stoppage of the gauge in any other way than that which he had mentioned.

Mr. L. F. Vernon-Harcourt said the author had remarked that the silt which came into tidal rivers was chiefly due to the tidal water. He did not see why tidal rivers should differ in any respect from non-tidal rivers in regard to the silt brought down by the fresh water. In many non-tidal rivers a great deal of silt was brought down. The Mississippi, the Nile, and the Danube, were charged with large quantities of silt, and it ought not, therefore, to be accepted as an axiom that the fresh water of tidal rivers did not bring down silt. No doubt the flood tide introduced a certain amount of silt with it, but a great portion of it was removed on the ebb. There was, however, a great deal of truth in what the author had said, that the tidal water alone of the ebb was not able to remove the whole of the silt brought up by the flood, and that the fresh-water floods did help to scour the channel. But there was another reason why the tidal waters were equally muddy with the fresh waters—the fresh water was constantly flowing past any given point to which it never returned, while the tidal waters were oscillating to and fro, and therefore a portion of suspended matter might pass the same point several times in succession. In the case of the Clyde, for instance, some flotation experiments had been made by Mr. Ure many years ago, to ascertain what happened with the sewage in the river. Some floats were put in and watched day and night from boats, and he believed it took nearly a fortnight for a float to go from Glasgow to the Firth of Clyde. If that were so the whole of the river between Glasgow and the Firth of Clyde had the fresh-water flow of a fortnight mingled with the tidal waters, and therefore it held all the amount of sediment in those 12 miles contained in that flow, and it was totally different in that respect from the fresh-water flow. It was an old proverb that a rolling stone gathered no moss, and he thought it might be considered an engineering proverb that moving water deposited no silt. When tidal water oscillated to and fro there was very little deposit of silt, but deposit began to take place directly stagnation occurred.

The harbor of Southwold had been mentioned by the author as a peculiar case of an entrance being kept open by the ebb and flow of the tide. A reference to the past history of the French and the Belgian harbors on the North Sea, would show that exactly the same thing was occurring; they were kept open by the ebb and flow of the tide, and when the marshy tide-covered lands surrounding them were reclaimed, as they had been by degrees (as at Ostend, Calais, Dunkirk, and other harbors), the en-

trances became partially blocked up, because the sea had a tendency always to close up the channels in forming a continuous line of beach along the coast. The author had also referred to Rye harbor, and did not seem to think it was improved by the blowing up of the sluice. Mr. Vernon-Harcourt believed that sluices in the form of dams had done harm to tidal rivers. There was the well-known case of the Grand Sluice near Boston. In summer the mud was sometimes heaped up 10 or 11 feet against the sluice doors; that would entirely prevent any navigation of a permanent kind going on in the summer, and though no doubt it was scoured out by the floods of the Witham in the winter, it was useless to have an entrance which was blocked up in the summer. There was also the well-known Denver sluice which had been blown up, with regard to which Sir John Rennie had mentioned that the outfall of the Ouse below was much improved during the time the sluice gates were removed. The author said he could find no evidence, or hardly any, as to the bad effect of dams across a stream, and he quoted Mr. Bouniceau, in support of his views. He had a great respect for Mr. Bouniceau, as gathered from a perusal of his book; and curiously enough, though that author agreed certainly with one of the author's premises, he did not agree with his conclusions. He had made a diagram (Fig. 11) taken from Mr. Bouniceau's work, in which he had shown two rivers, the Vire and the Aure. They discharged into the English Channel through Vays Bay, situated between Cherbourg and the mouth of the Seine, and the tide was excluded from the rivers by gates across the arches of the Vay and Isigny bridges many years ago. The two dams were placed there in order to protect the lands from the tidal floods, and it was soon found that the estuary began to silt up, and that whereas vessels were formerly able to go to Isigny, boats of a much smaller tonnage were not able to get up there; and the Vire was also silted up to a considerable extent. Mr. Bouniceau gave the following extract, from the report of a commission of inquiry on the subject, in support of his opinions, in the same chapter from which the author had quoted: "The flood-tide in rising up the Vire brought up a volume of over 3,000,000 cubic yards, which flowed four times a day through Vays Bay. It was therefore traversed by a volume of water amounting to 13,000,000 cubic yards in the twenty-four hours. At the present time it is only traversed by the waters of the Aure and the Vire, which together discharge merely 954,000 cubic yards in a day. It is therefore undoubtedly the Vay bridge which has destroyed the maritime navigation, solely because it has flood-tide gates which have prevented the continual oscillation of the flux and reflux of the tide." There was a further history with regard to those rivers. The authorities were not able to take away the dam at Isigny for local reasons, but they took away the gates from Vay bridge on Mr. Bouniceau's recommendation; and Mr. Bouniceau said that as soon as the dam was taken away the navigation of the Vire was restored to its original condition. That proved plainly enough that the author was not justified in asserting that dams had no effect in reducing the navigable channel of rivers. The author had referred to the question of the embankment of tidal rivers, as a system which the modern school of engineering would condemn and forbid. That was a very difficult question, but he did not think the modern school of engineers were quite agreed that the embankment of rivers should not be carried out. The Seine was a case where embankment had been carried out to a large extent within recent years, with advantage to the river above but injury to the outfall below; and the works on the Meuse below Rotterdam furnished another instance of modern river embankments. He would not then enter into the question, as he had offered a paper to the Institution treating of that subject and of the maintenance of harbors on sandy coasts, but there were several rivers in England, like the Clyde and the Tees, which had been embanked. He thought that the question was not whether rivers should be embanked at all, but to what extent they should be embanked. He considered that the exclusion of the tide from any river would prove fatal to its proper maintenance by causing stagnation and consequent deposit in the summer, and that, on the contrary, the tidal flow should be admitted as far up a river as possible to pro-

FIG. 11.



duce as great a tidal oscillation as practicable at the mouth. He hoped the result of the discussion would be to prove that the author, though right in some of his premises, was not justified in the conclusions at which he had arrived.

Mr. George Higgin remarked that the conclusions arrived at in the paper appeared to be based principally, if not entirely, on the relation existing between surface and bottom velocity. The author appeared to consider that the ratio of bottom to surface velocity increased rapidly as the depth decreased. On a given inclination, therefore, it would follow by this theory that the shallower the water the greater would be the bottom velocity and the greater the scour; hence, the author's belief that the scour of a channel was entirely produced by what he called the "low-water flow." The author supported this theory of bottom velocity by some experiments made by Graeve, on the Oder, by others, by General Ellis on the Connecticut river, and by others again, by Mr. Shaw, under his own directions, on the Avon. He entirely agreed with the opinion already expressed, that these experiments were insufficient to support so startling a theory as that put forward; a theory, it need scarcely be said, entirely contrary to all former ones. He would wish, however, to call attention to the result of the experiments by M. Révy on the River de la Plata. The author appeared to have entirely misconceived M. Révy's experiments, which, so far from proving his theory, proved exactly the contrary. The experiments referred to in the paper, were made by M. Révy, in a place known as "the outer roads," in the River de la Plata, at a distance of about 4 miles from the shore. The river here had a width of

about 30 miles, and the point selected was the deepest part of the channel. The bed of the river from this point to the sea, a distance of 120 miles, was almost horizontal. The experiments were made on the ebb tide. The first day's experiments showed that at this point the bottom velocity bore to the surface velocity, which in this case was the greatest, a proportion of 33 to 100. The depth of the water under the platform was found to be 22 feet. On the following day M. Révy returned to the same platform and continued his experiments; he was surprised to find that with a slightly decreased surface velocity, the mean and bottom velocities were greater than those of the former day, the bottom velocity being on the second day 42 per cent. of the surface velocity, instead of 33 per cent. He ascertained that this increased ratio was due to the greater depth of water, for, whereas, on the first day there were 22 feet of water under the platform, on the second day the depth was 23 feet 8 inches. Other experiments, made higher up the river, in a depth of 50 feet of water, and with a very similar surface velocity to the other ones, gave the bottom velocity as 70 per cent. of the surface velocity. M. Révy's experiments, which were conducted in a most careful and scientific way, led him to lay down several rules:

(1) That with a given inclination the surface velocities varied directly as the depths.

(2) That the mean velocity was an exact proportional between the surface and bottom velocities.

(3) That the mean and bottom velocities increased in a greater ratio than the surface ones.

In fact, M. Revy's experiments seemed to prove that on a given inclination, whilst the surface velocities varied directly as the depths, the bottom velocities varied as the squares of the depths; and that probably at the depth of about 71 feet the top and bottom velocities were practically the same. These conclusions were exactly contrary to those put forward in the paper.

He might state, as a matter of interest in connection with the silt-bearing powers of rivers, that he had ascertained from careful experiments at Buenos Ayres, that the amount of suspended matter carried past that city by the River de la Plata every twenty-four hours, in ordinary states of the river, exceeded 212,000 cubic yards.

Mr. Walter Browne, in reply, said he was quite aware that the paper would encounter a large amount of opposition, and he had no intention of complaining at the way it had been received. Apart from individual criticisms, there were two general lines of argument that appeared to have been taken in objecting to his conclusions. The first was that they were contrary to received opinions; but he presumed that the Institution existed for the progress of engineering, and the progress of engineering, like the progress of all other things, had generally been over the bodies of the received opinions of former generations. The second argument was that his experiments were not reliable.

The remarks of Mr. Shelford might be summed up in one, that the existence of deltas was a sufficient proof that Mr. Browne's conclusions were wrong; because deltas were formed of mud brought down by rivers, and that they were only formed in tideless seas where there was no beneficial tide to sweep them away. Now what was meant by a delta? Simply that a river found its way into the sea by more mouths than one; and what that necessarily had to do with the question of the amount of silt which the river had or had not brought down, was not at first sight very easy to see. The question which really underlays the whole matter was, not what cause which had originally brought the silt into any particular place, but, supposing the tidal flow up and down the river to be taken away, would the silt remain the same, or would it be diminished? and to that question the existence of deltas gave no answer. But Mr. Shelford's argument as to deltas was deficient in its premises. He had rested it upon the allegation that deltas existed in tideless seas, and those only; and he quoted three cases of deltas—the Danube, the Nile, and the Tiber, all of which undoubtedly existed. But he might point out that not far from the mouth of the Danube there were three great rivers, the Dnieper, the Dniester, and the Bug, which flowed into the Black Sea (a tideless sea), but had no deltas. And on the other hand, there were rivers which flowed into

tidal seas and yet had deltas. He might point to the Ganges—all had heard something of the tide flowing up the Hooghly—to the Irrawaddy, to the Yang-tsi, to the Cambodia, and to the Indus, in Asia; in Africa, to the Zambezi and the Niger; in America, to the Mississippi, the Orinoco, and the La Plata. Those were some of the largest rivers in the world, all flowing into tidal seas, and all flowing through deltas. Mr. Shelford further said that rivers could not keep themselves open, because the mouths of the Tiber were shallow, whereas that of the Thames was deep. It was hardly necessary to point out that if a river were split up into several branches, the size and depth of those branches must be less than the size and depth of a single channel would be, supposing the river to empty itself by one channel. Mr. Browne did not say that the fresh water would keep open just such a channel as engineers would like to see, but he did say that it would keep open just such a channel as agreed with the amount of the waters that flowed down.

One or two obvious mistakes had been made by Professor Unwin in quoting from the paper, as, for instance, where he stated that the velocity was taken as proportional to the depth, instead of proportional so the slope; whereas it was really taken proportional to the depth at one particular place, at which the depth was proportional to the slope. Again, he called it absurd to say that silt was deposited most thickly in the deepest water, because in the deepest water the velocity was quickest. Mr. Browne had thought that silt was deposited, not when the water was running rapidly through a channel, but when the water was at rest, and that, in consequence, it would be deposited most thickly where there was the highest column of water from which it might fall. He was not directly answerable for the paragraph noticed about large rivers; he had simply compiled it to complete what appeared to be the facts of the case as far as he could ascertain them, and he still considered that it was substantially true. Professor Unwin had especially objected to Humphreys and Abbot's experiments on the Mississippi, as being supposed to prove that the velocity near the bottom in very large rivers was the same at the top. But Mr. Browne had been careful to say that it was not exactly the same, but that the difference was small. He had since again consulted their report, and he found that the average bottom velocity was about 90 per cent., not of the surface velocity, but of the greatest velocity, which was always at some depth, generally at a considerable depth, below the surface. No doubt in measuring the discharge of a river, that 10 per cent. of loss was a consideration to be taken into account; but it was no consideration whatever in the argument as to scour, because the scour was practically the same, whether a river flowed with a velocity of 5 feet or of $4\frac{1}{2}$ feet per second. His experiments had nothing whatever to do with small differences of velocity, and any arguments based on such differences, for instance as to the meter not being sufficiently delicate, were completely out of place.

With regard to Mr. Gordon, who, as Prof. Unwin had said, had made many thousands of observations, and was still making them (as he himself knew from recent correspondence), were they all to go for nothing, because they could not be made to fit in to the theories of a French hydraulician, who could never have seen a river bigger than the Loire or the Seine, rivers which, as compared with those where Mr. Gordon had experimented, were nothing more than ditches? He had himself had occasion to compare at great length the formulæ of different hydraulicians on a kindred subject, and the discrepancies and confusion then revealed had brought him, while thankfully accepting the facts hydraulicians had to offer, to place little reliance on their general conclusions or theories. Now, the facts which Professor Unwin had introduced were only two in number. One simply amounted to this, that in the particular cases cited, in the lower curves especially, there was practically little or no time between the lowest point of one ebb and the beginning of the rise of another flood. Every one knew that that was the case in the sea outside a river, and perhaps in a wide estuary; he should have thought that everyone also knew that it was not generally the case any considerable distance up a river. To anyone who, like himself, had been accustomed to work on such a river as the

Avon, where one could regularly count on two to three hours of work at low water, during which time the level practically remained constant, that argument did not commend itself as a common-sense view of the question. The other fact referred to the proportion between the tidal and the low-water flow. It really required no diagram to convince anybody that the amount of tidal water running up and down a river was greater than the amount of low-water flow; in all cases it was much larger, and in many cases a great many times larger, than it was shown in the diagrams, Fig. 4. It was, however, difficult to suppose that Professor Unwin could seriously maintain that the scour of the bottom of a river channel was simply proportional to the amount of water running up and down through that channel. With regard to the suggestion that the velocity at any particular point of a river varied from time to time considerably, that might be true, at least in some cases; but it could not affect his experiments, which gave the average velocity as zero. The only other remark made by Professor Unwin which needed notice was that the bottom of a river might be scoured by the tide, while the sides were being silted up. If that were so, all rivers were tending continually to become narrower and deeper, but he was not aware of any facts proving that that was the case. The whole question was, whether the rise and fall of the tide did, in fact, sweep away more mud than it deposited. Now, supposing that, on the whole, it swept away $\frac{1}{1000}$ inch in a single tide, and thus increased the depth $\frac{1}{1000}$ inch, which was as little as one could well put it at, at that rate in four hundred years the bottom of a river would be lowered 25 feet; and he should like to know what evidence there was that during the last four hundred years the bottoms of rivers had been deepened to that extent by the action of the tide. Mr. Browne's way of looking at the matter was this: There were two forces at work; there was the water which came down from above (he was speaking of tidal channels in general, excluding special cases), and there was the water which went up and down with the tide. The way to find out which of those was doing the work was surely to examine the cases where they acted separately. Now, he knew of no case where a stream ran down into the sea, and where it had become completely silted up and unable to keep its channel open. Nor did he know of a case where the fresh water had been taken from a channel, turning it into a creek, and where (of course, in muddy estuaries or muddy seas) that creek had not begun, more or less rapidly (generally very rapidly) to fill up. What did this mean, except that the tidal water tended to choke up the channels, and that the fresh water tended to keep them open?

He was obliged to Mr. Redman for confirming his views as to the silt coming up from the sea and accumulating on the shores, but he altogether repudiated the proposition attributed to him, that the net amount deposited by the tides and the net amount scoured by the fresh water must be in proportion to their respective volumes. He must repeat that the volumes had nothing to do with the question, nor had any reason been given for supposing that it had. He would not go into the question of the maintenance of Harwich and other harbors, because the facts relating to those cases were not under discussion, but Mr. Redman had saved him all trouble by disproving his own case. He wanted to show that the depth of harbors was due to the amount of tidal water which passed in and out, and two instances adduced were Yarmouth, having 6 million tons of tidal water, and Montrose, having 40 to 50 millions, which had just the same depth; where was the proportion between depth and volume in those cases? With regard to Mr. Richardson's view that tidal channels were, in general, proportional to the upland waters, Mr. Richardson expressly spoke of moderate tidal channels, such as those of the streams falling into the Bristol Channel, not such as the Bristol Channel itself, which was practically a part of the ocean. The large estuaries mentioned by Mr. Redman fell into the latter category. In the paper he had expressly excluded the consideration of large estuaries, on the ground that he had not been able to find any observations concerning them.

A complaint had been made by Mr. J. Thornhill Harrison that the experiments did not extend to a variety of points at which he should have liked information;

Fig. 13

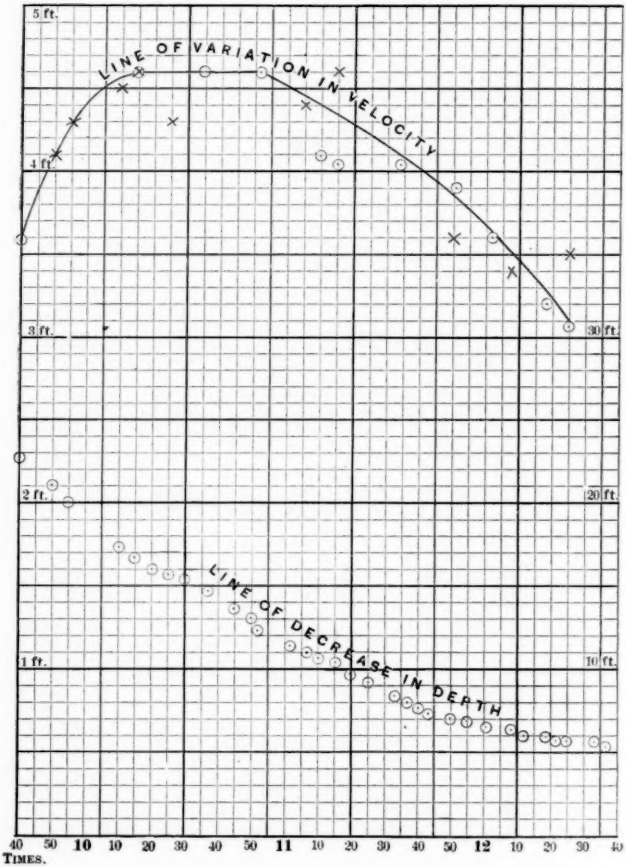


Diagram of experiments on surface velocity in the Avon, September 21, 1880.

N.B.—⊙ denotes velocity determined by meter at surface.

× " " " float, "

but Mr. Browne thought it was better to restrict experiments to the one point to be solved, and which, in this case, was the ratio of bottom velocity to surface velocity, at a certain spot selected, so as to give a fair average result. With regard to the accuracy of the meter, the surface observations for the second series of experiments, both for meter and floats, were plotted together on Fig. 13, and it would be seen that the meter observations were much the more regular of the two. Except for the two observations at 11.10 and 11.15 A. M., which seemed to be too low, they could not be improved. The line drawn was a fair average of both sets of observations, and it exactly answered to the description in the paper. On the same diagram, lower down, he had plotted the observations on depth, and it would be seen that they showed a very steady fall up to about 12 noon, when the rate diminished suddenly, and what he called the low-water period began. He could not follow Mr. Harrison in his views as to the way in

which the water fell in the river. Even at Netham Dam, itself, there must be horizontal motion for the water to get away, without taking any account of the fact that there was fresh water coming over the dam to help it. Mr. Harrison wished to argue that that dam ought to be removed, but that dam was a standing instance of the truth of Mr. Browne's views. There was no doubt that, when put up, it interrupted the tidal flow, and, therefore, on his opponents' principles, it ought to have produced silting up in two parts—above the dam, where the mud brought down by the river would have to settle, and below the dam in the New Cut, where there would be a diminished tidal scour to keep the channel clean. Now there was a good deal of dredging done at Bristol, but it was never done and never required at either of those two places. On the contrary, the places where it was chiefly required were in the half-tide basins, and at the lower end of the floating harbor where the tidal waters entered, and where there were no upland waters to counteract their evil effects. That was a simple fact, to which he spoke from full personal knowledge. Fortunately for Bristol, Mr. Jessup, who constructed the floating harbor and the Netham Dam, had ignored the silting-up bugbear, and his work had been crowned with complete and enduring success.

Some objection had been made by Mr. Law to the term low-water flow, which had already been explained. Mr. Law had also described an elaborate form of meter, which appeared to be valuable, and had mentioned some experiments, of which, as they were not described, it was impossible to say whether they were valuable or not. With regard to the objections taken to the ordinary form of meter, Mr. Law would see, on reading the paper, that none of them applied to the instrument used by Mr. Shaw.

The facts brought forward concerning the Scheldt by Mr. Russel Aitken naturally ranged themselves on his side of the question. He could not, however, agree with the view that differences in the temperature of the water affected the question; in a river like the Avon such differences would be quite insignificant.

With respect to Mr. Vernon-Harcourt's observations, it was, of course, true that some rivers brought down a good deal of silt, but as, unlike tidal waters, they were always in motion, they did not, in general, deposit this silt, at least until they reached the sea, where it often formed sand banks, &c. The idea that the oscillations backwards and forwards of the water in the tidal area kept it more muddy, was, however, a fallacy. The whole fresh-water discharge must get out somehow in the course of each tide, otherwise the river would rise and overflow, and in getting out would carry its silt with it. The case quoted of a sluice near Boston was an unfortunate one, because the fact that the accumulation took place in summer, not in winter, clearly showed that it was due to the tide, and that the scouring power of the fresh water behind the sluice was not used to clear it away. The case of the Vire, mentioned by Mr. Harcourt, had not escaped him. It was a good illustration of the circumstance described in another part of the paper. The tidal waters passing up the Vire, and overflowing a large area of waste land on both sides, greatly increase the flow at the end of the ebb, and when they were dammed off, the bay outside the mouth of the river began to silt up. No doubt that might be prevented to a great extent by judicious scouring, but at any rate it was not a case of injury to a navigable channel, as Mr. Vernon-Harcourt had said, but to a bay or harbor.

A doubt had been expressed by Mr. Atkinson about the expediency of "dock-izing" rivers, because he thought it must cause silting both above and below the dam, but experience showed that the former was quite insignificant (at least in ordinary cases), and that the latter would be perfectly dealt with by scouring. Of this the new Avonmouth docks were a striking instance; the entrance lock there (and indeed every entrance lock) formed exactly such a dam, and there was no fresh water flowing through the dock. It was prophesied that the entrance would silt up, but by simply using the impounded water to scour with at low tide (thus creating a spurious low-water flow) it was easily kept clear. If that lock were permanently opened, and the dock left to the mercies of the all-powerful tide, it would be silted up in a year or two.

The case of the Seine was nearly parallel to that of the Vire; but Mr. Shool-

bred's illustration from the Dee was most unfortunate. It appeared that a large quantity of land on that river had been embanked with success, and during that time no complaints were made as to damage to the navigation. But when a further attempt was made, which was a failure, the tide breaking in, and washing over the reclaimed land (as he had himself seen it), then it appeared that the navigation was injured. Whatever might be the cause of that injury, one thing was absolutely certain, that it could not be the shutting out of the tide, simply because the tide was not shut out.

The remarks of Mr. Giles on the Thames at Gravesend, and the Mersey at Liverpool, referred to large estuaries, which had been expressly excluded. With regard to the Clyde, the "correcting" of a river was a totally different thing from embanking it as to diminish the tidal area, as had been done so successfully in the Thames, Severn, &c. The improvement of the Clyde was, no doubt, immense, but it had been achieved by what might be called the brute force of dredging, and that dredging would have to be continued, or the channel would silt up again; whereas, were it turned into a ship canal, as Smeaton had advised (of course on a much larger scale than he suggested), it would keep open.

He accepted, of course, Sir John Coode's correction as to the cause of the travel of shingle along the East coast, which was quite immaterial to the argument. With reference to Mr. Higgins' remarks, it would perhaps have been better not to quote M. Révy, whose experiments were mainly conducted in very large rivers; and in these, the ratio between the bottom and surface velocities appeared to have a character of its own.

CORRESPONDENCE.

Mr. Laroche would confine himself to a consideration of the two alternatives virtually put forward by the author, *i. e.*:

(1) The entire closure of a tidal river by the construction of a transverse dam, so as to shut out the tide from the part above the dam, thus converting it into a floating basin.

(2) The longitudinal embankment of the sides of the river.

In every tidal river with which he was acquainted, if the channel were barred completely by a transverse dam established within the course of the river, *i. e.*, at a point notably above its mouth, as at A A, Fig. 14, the following effects would be produced. If Fig. 15 represented the primitive longitudinal section of the river at low water, this profile would, in a very few years after the construction of the dam, he modified to that shown in Fig. 16. An enormous alluvial deposit from the sea bed would occur at C, tending to reach to the level of low water, and even above it. This would happen below the dam; above it, also, a deposit of fluvial silt would be

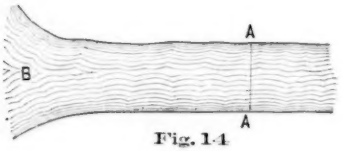


Fig. 14

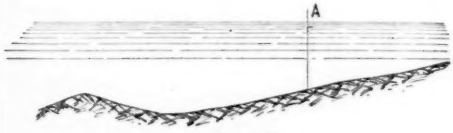


Fig. 15

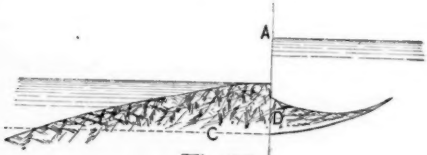


Fig. 16

produced, proportionate to the mean amount of the land water brought down in floods. If, now, it were desired to establish a port above the dam, it would be necessary to cut a channel through the shoal C. To effect this solely by natural action, without having recourse to dredging, the head of water retained by the dam would alone be available. There would apparently be no more efficient way of unsilting the required channel than by allowing the dammed-up water to escape violently at low water. But the effect of this mode of erosion was well known. Water thus escaping soon lost its impetus, the only result being

that when the flushing sluices were opened the deposit was removed from the vicinity of the dam to accumulate at a point more to seaward, as shown in Fig. 17.

If this proposition were as true as he thoroughly believed it to be, it was difficult to see how the condition of a port situated above A could be bettered by the establishment of such a dam. In reality there would have been formed a vast reservoir of fresh water above the dam, but access to it would have been made, if not impossible, at least much more difficult than was access to the river before the dam was constructed.

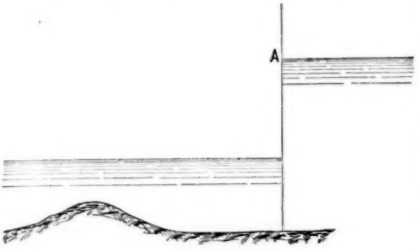


Fig. 17

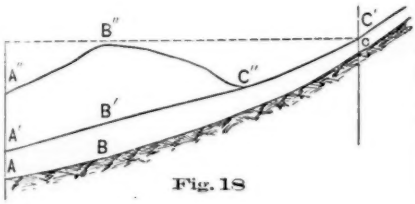


Fig. 18

As regarded longitudinal banks, he would remark at the outset, that of all the important tidal rivers which he knew the land water as a rule formed a very small part of the volume as compared with the sea water, the only exception being the floods brought down at certain known seasons, and for a few days at each season. If ABC, Fig. 18, represented the river bed (A being in the tidal and C in the fluvial part), A′B′C′ the surface of the water at its lowest, and A″B″C″ the surface when the tidal wave, after having expended itself, began to acquire perceptible motion towards the sea throughout the whole depth AA′A″, the whole volume of water comprised between the surfaces A′B′C′ and A″B″C″, added to the fresh water coming down at CC′, would have to escape by the section AA′A″, of which the height AA′ would be constantly diminishing. Now, he had observed in the tidal rivers he was acquainted with the summit of the tidal wave B″ in the river maintained sensibly the same elevation as that of the tidal wave in the open sea in front of the river's mouth. The volume A′B′C″B″A″ would therefore be greater in proportion as the river was wider above the point of low water, A′A″. And as this volume had to pass off at AA″ in a period (generally a little more than six and a quarter to six and a-half hours, occasionally seven hours) depending above all on the condition of the tideway below AA″, the speed would be greater in proportion as the volume having to escape (within limits of time of small variation) was itself greater. This volume would also be greater in proportion as the basin filled by the tide above AA′ was greater, that was, as its depth and width above the point of low water were greater. The bottom-velocity augmented rapidly from the time it became sensible, in proportion as the water fell. Therefore the channel of a tidal river should not be reduced above the point A, lest the depth might be decreased at that point; and it followed that the sides should not be embanked. It would even be better to enlarge the capacity of the basin, if regard had only to be had to the question of depth at A.

To sum up, he was not in favor of embanking tidal rivers if it were only a question of increasing the depth of water; for there was too much reason to fear that an opposite result would be obtained. He insisted on the restrictive conditions imposed in the problem as he approached it, because in practice the objects in view were never so simple as he had supposed. The embankment of certain places covered by the tide might be imposed by circumstances altogether foreign to the present subject. He would repeat that his conclusions were solely based on two points observed by himself: (1) That the top of the tidal wave in a river was sensibly the same as that of the same wave in the sea. (2) At a given point in the tidal portion of a river the duration of the ebb depended principally upon the condition of the

tideway below that point, and that it varied within very small limits when the river was in its usual state. It followed that the width of the basin covered by the tide above the point in question had as a rule little influence on the height of the tidal wave or the duration of the ebb tide. The volume of water brought into this basin had, on the contrary, a very great influence on the velocity of the ebb at the point under consideration, that was to say, below the basin. It was evident that where these two conditions did not occur his conclusions would not apply; but these facts had been demonstrated in a certain number of tidal rivers he had observed, and he was disposed to accord to them a probable character of generality.

It might not be out of place to remark also, that if at the same time that the sides of a river were embanked the bottom were dredged and rectified, the disadvantages of the first course of treatment would be largely compensated, and the combination of the two systems would greatly facilitate the propagation of the tidal wave. The level of low water in the upper part of the river would be lowered, and the capacity of the reservoir to be emptied at each tide augmented, to the great advantage of the maintenance of the depth in the lower channel. In a word, if a compensation had been offered for the embankment of the width of the bed by the increase of its depth, and by rectifying its course, such treatment would induce a modification in the conditions at once considerable and advantageous.

ELECTRIC CONDUCTIVITY OF METALS AND THEIR ALLOYS.

By M. LAZARE WEILER.

Translated from a Communication to the Société Internationale des Electriciens.

In the study of all electrical problems, and especially of such as relate to dynamic electricity and its applications, we are brought at the outset to the consideration of conduction.

Whether it be a question of light, of force, of heat, of chemical or mechanical phenomena, or, in a word, of any transformation whatever of electricity, it is indispensable to know the quantity of electricity produced at the source, and the amount afforded at the place where it is utilized.

The nature of the intervening conductor becomes then one of the principal elements of the problem.

In the dynamo-electric machine which is only a conductor or a series of conductors subjected to magnetic induction, the property of conductivity is an important one.

The properties, therefore, of different metals and their alloys, as regards their power of transmitting electricity, afford a subject of exceeding interest.

This question furthermore merits our attention now, for the reason that physicists who have heretofore investigated it have failed to arrive at concordant results, either because of the impossibility of obtaining absolutely pure metals, or by reason of the imperfections of the apparatus or the methods for electrical measurement at the date of their researches.

When Mathiessen made his experiments (1863) it was extremely difficult to procure certain metals in a pure state. He mentions the labor necessary to produce 5 or 10 kilograms of pure metal, while now it is a matter of daily occurrence in some manufactories to deposit by electrolysis, metals in a pure state by the ton.

The processes employed in the establishment of the Nordeutsche-Affinerie, at Hamburg, as well as certain works in Belgium and England, lead to the extraction and separation of the metals from argentiferous copper ores, which contain small quantities of silver and traces of gold, not capable of separation by the older methods.

This is profitably applied to ores in which the presence of the precious metals did not formerly affect the commercial value.

Under such conditions it becomes easier and all the more necessary to review and complete former researches. This is

the work that I have been able to undertake by aid of the special facilities afforded me by a carefully fitted laboratory, and a workshop in which are applied on a large scale, processes which were not formerly applied outside the chemist's laboratory.

I shall have the honor to resume before you the researches which I have pursued for some years. The results obtained are far from being complete, but they permit us to closely examine certain points of the theory of a subject upon which our ideas are not yet conclusively formed. They afford in all cases useful corrections to the published tables, which are for the most part afflicted with errors.

This double claim is, I trust, sufficient to engage your kind attention.

Researches relative to the conducting power of different bodies have been pursued only in recent times. Priestley was the first to attempt the determination of the conducting power of metals for a static charge of electricity.

There was no precision in his method of comparison. He employed two wires of the same length and diameter, but of different metals, and sent charges through them until one was fused. The fused wire was considered the poorest conductor.

Davy applied the same general method with voltaic currents, and succeeded in establishing the law relative to the dependence of electric resistance upon the length and cross-section of the wire.

M. E. Becquerel, who has written a memoir on the origin of these researches, gives the table of relative conductivities, as measured by his father, who was the first physicist to engage seriously in this labor.

The table is as follows:

Copper	100
Gold	93.60
Silver	73.60
Zinc	28 50
Platinum	16.40
Iron	15.80
Tin	15.50
Lead	8.30
Mercury	3.45
Potassium	1.33

Since that time Pouillet has given the same measures and added the following table, which possesses a special interest from the fact that it exhibits the influence of the presence of foreign bodies, also of tempering and annealing.

Palladium	5791
Silver (963 fine)	5152
" (900 ")	4753
" (857 ")	4221
" (747 ")	3882
Gold, pure	3975
" (951 fine)	1338
" (751 ")	714
Copper, pure	3838
Copper, annealed	3842
Platinum	855
Brass	200 to 900
Cast steel	200 to 500
Iron	600 to 700
Mercury	100

The selection of a unit for comparison in this table is a matter of some interest.

Pouillet has chosen mercury for his standard obviously for the reason that being a liquid, it is the metal most easily obtained in a pure state, and that also, from this same property of liquidity, it is exempt from those variations of conductivity which arise from annealing, hardening, etc.

These considerations prompted the Congress of 1881 to select mercury for the material of the standard of resistance. At a second reunion of this Congress, this selection was approved, and the *ohm* was defined to be that amount of resistance offered by a column of mercury whose area of section is one square millimeter, and whose length is 106 centimeters; the temperature being 0° Centigrade.

The important researches of Becquerel, in 1846, led to the determination of the relative conductivity of several metals at the mean temperature of 12.75°.

These results are exhibited in the following table:—

	Hammered.	Annealed.
Silver, pure, reduced from chloride	93.448	100
Copper, precipitated by battery and fused	89.084	91.439
Gold, pure	64.385	65.458
Cadmium	24.574	
Zinc	24.164	
Tin	13.656	
Palladium	12.977	
Iron	12.124	
Lead	8.245	
Platinum	8.042	
Mercury (14°)	1.8017	

Mathiessen afterwards published in the *Proceedings of the Royal Society* the

following table, which has been accepted for practical use down to the present time:

	Resistance in ohms of a wire 1 m. long, & 1 mm. diam.	Conductivity.
Silver, annealed	0.01937	100.
" drawn	0.02103	92.1
Copper, annealed	0.02057	94.1
" drawn	0.02014	94.1
Gold, annealed	0.02650	73.
" drawn	0.02697	73.
Aluminum annealed	0.03751	51.6
Zinc, rolled	0.07244	26.7
Platinum, annealed	0.1166	16.6
Iron, "	0.1251	15.5
Mercury, liquid	1 2247	1.58
Nickel, annealed	0.1604	12.
Tin, pressed	0.1701	11.4
Lead, "	0.2526	7.6
Antimony, pressed	0·4571	4.2
Bismuth, "	1.689	1.1
ALLOYS.		
Platinum 1, Silver 1, (drawn or annealed).	0.3140	6.1
German Silver, (drawn or annealed).	0.2695	7 2
Gold2, Silver 1	0.1399	13.8

An examination of these tables shows a want of accordance, not only in the relative conductivities of the different metals, but also in their order. Thus silver occupies the third place in the table of the elder Becquerel, whereas it should have been in the first. Also Palladium is in the first place in Pouillet's table, while it should have been placed last.

These differences justify the new researches, which are furthermore prompted by that desire for precision which the daily progress in the practical application of electricity renders necessary.

I now proceed to present a *résumé* of my researches in determining the resistance of metals and alloys: then the results of experimenting upon the influence of temperature, and finally, upon the relations more or less intimate between the electric and thermic conductivity. I shall hand to you a set of small bars which have been prepared for these experiments. These bars, as you see, have been cast with a diameter of about 13 millimeters. They have been cut in such a manner as to exhibit the grain of a fracture, and the detached portion has been drawn into wire, upon which the experiments now to be described were tried.

In case of the alloys which cannot easily be drawn or rolled, as some of the compounds of silicon or phosphorus, the experiments have been tried directly upon the bars themselves by the method of Sir William Thomson.

In conducting the experiments, the bars were supported upon V-shaped rests, placed at an invariable distance apart. These rests were respectively in communication with two resistances composed of two parts, of which one is one thousandth of the other.

A Wheatstone bridge, a differential galvanometer, a battery of four cups, and a contact key complete the apparatus.

The measurements, which were very long and minute, and made upon a great number of specimens, were made in part by the writer at the workshop at Angoulême, with the assistance of Messrs. X. Muller and J. Stahl, engineers of the establishment, and partly by M. Duflon, electrical engineer in the measuring room of M. Sciami, director of the Maison Breguet.

These latter experiments were those made upon the bars themselves.

The number of specimens submitted to your examination is thirty-four. Their conductivities compared with silver and pure copper are given in the following table.

Silver	100
Copper, pure	100
" refined and crystallized	99.9
Bronze, silicious, telegraphic	98.
Copper and silver alloy; eq parts	86.65
Gold, pure	78
Copper with 4 per cent. of silicon	75
" " 12 " "	54.7
Aluminum, pure	54.2
Tin with 12 per cent. of sodium	46.9
Silicious bronze, telephonic	35.
Copper with 10 per cent. of lead	30.
Zinc, pure	29.9
Phosphor-Bronze, telephonic	29.
Brass, silicious, 25 per cent. of zinc	26.49
Brass with 35 per cent. of zinc	21.5
Tin phosphide	17.7
Gold and silver alloy; eq. parts	16.12
Swedish Iron	16.
Tin, pure; banca	15.45
Antimony-Copper	12 7
Aluminum-Bronze	12.6
Siemens steel	12.
Platinum, pure	10.6
Copper with 10 per cent. of nickel	10.6
Cadmium 15, mercury 85	10.2
Bronze, Mercurial, dronier	10.14
Arsenical copper; 10 per cent. arsenic	9.1
Lead, pure	8.88
Bronze, containing 20 per cent. tin	8.4
Nickel, pure	7.89
Phosphor-Bronze with 20 per cent. Tin	6.5
Copper with 9 per cent. phorphorus	4.9
Antimony	3.88

A large number of experiments were tried upon alloys and compounds not represented in the above series.

It will be observed that in the results recorded above, resistances are not given in ohms, but conductivities are expressed relatively to a selected standard, which is pure silver. A wire of this metal, one millimeter in diameter, offers a resistance at 0° Centigrade of 19.37 ohms per kilometer.

Pure silver and pure copper are the best known conductors of electricity. The ease with which silver can be obtained in a pure state would seem to entitle it to the position of standard of comparison. It also oxidizes with greater difficulty than copper, and its oxide is a better conductor. In this last peculiarity, there is less chance of error when comparisons are made with silver for a standard.

Gold has a conductivity of 78. The alloy of gold and silver calls attention to a remarkable fact. When silver is alloyed with a small percentage of gold, the conductivity immediately diminishes. 2 per cent. of gold is sufficient to reduce the conducting power from 100 to 60. An increase in the proportion of gold, however, is attended with a decrease of conductivity at a less rapid rate, the minimum of 16.12 being the conductivity of an alloy of equal parts of gold and silver.

If the varying conductivities of the different proportions of gold and silver alloy be represented by a curve, it will be found to take the form of a parabola whose axis is perpendicular to the axis of X. The curve descending from the ordinate 100 for pure silver to 16.12 for the alloy of equal parts of the two metals and rising to 78 for pure gold.

This singular phenomenon seems to indicate that a kind of interference is produced analogous to that when rays of light partially extinguish each other by superposition. This is not an explanation, but a simple analogy.

It should be remarked, however, that it is not true, though sometimes asserted, that in an alloy the electric conductivity is always lower than that of the poorest conductor of the constituents. It is simply demonstrated that the union of two bodies modifies to a great extent their separate conductivities, and this fact ought certainly to lead some time to interesting results.

In examining the foregoing table, we find platinum with a conductivity of 10.6. Aluminum, which is distinguished from the other metals by its want of density, has the considerably higher conductivity of 54.2. This is partly compensated for by its low specific weight. But it is a mistake to consider its lightness as a quality fitting it some day for aerial telegraph lines. Its tenacity is rather feeble, and its tensile strength falls from 10 kilograms per square millimeter in the hard metal, to 6 or 7 kilos only in the annealed metal.

Iron, steel, lead, zinc, and tin, possess low conducting powers, and the results in the table are not insisted upon. It is sufficient to consider them in connection with results previously found.

Copper, however, is to be regarded with interest and care, as it, with its alloys, is regarded as the typical conductor of electricity. It possesses when unalloyed and is free from its oxide, a conducting power equal to silver. Its cost is low enough to render it available for industrial uses. It is malleable, and some of its alloys possess a remarkable tenacity.

It is interesting to note the increase in the conducting power of copper with the improvement in manufacture. In the late discussion upon electric conductors, Mr. Preece exhibited the following table, showing the progress in conductivity of submarine cables:

Submarine Cables.	Date.	Conductivity.
Dover-Calais	1851	42
Port Patrick-Donaghadee	1852	46
Transatlantic	1856	50
Red Sea	1857	75
Malta-Alexandria	1861	87
Persian Gulf	1863	89.14
Transatlantic	1865	96
Irish Sea	1883	97.9
Pure Copper	—	100.

A similar progressive increase in the conductivity of the copper wire, furnished to the Administration Francaise des Télégraphes, which, after having for many years exacted a conductivity altogether indeterminate, so to speak, has been gradually led to adopt figures quite comparable to the highest in our table.

The variation in the conductivity of copper is due to two principal causes: the presence of other bodies; and the presence of a varying quantity of copper oxide.

Mathiessen and Holzmann have made a series of interesting experiments bearing upon this subject.

The first table exhibits the conductivities of copper of different sections.

	Conductivity.	Temperature.
1st. Spanish copper from Rio Tinto, containing 2 % of arsenic with traces of oxides of copper lead and nickel.....	14.24 ..	14.8°
2d. Russian; Demidoff with traces of arsenic, iron and nickel oxides.....	59.34 ..	12.7°
3d. "Tough cake," location not specified, traces of lead, iron, nickel and antimony oxides..	71.03 ..	17.3°
4th. ——, traces of iron, nickel and antimony oxides.....	81.35 ..	14.2°
5th. Australian, Burra-Burra, traces of iron oxide only....	88.86 ..	14.0°
6th. American: Lake Superior, 3 % of silver, with traces of iron and copper oxide:.....	92.57 ..	15.0°

The second table exhibits the conductivity of copper containing traces of different substances.

Copper with	Conductivity.	Temperature.
2.50 % phosphorus.....	7.24 ..	17.5°
0.95 "	23.24 ..	22.1°
0.13 "	67.67 ..	20.0°
5.4 % arsenic.....	6.18 ..	16.8°
2.8 "	13.14 ..	19.1°
Traces of arsenic.....	57.8 ..	19.7°
3.2 % zinc.....	56.98 ..	10.3°
1.6 "	76.35 ..	15.8°
Traces of zinc.....	85.05 ..	10.3°
1.06 % iron.....	26.95 ..	13.1°
0.48 "	34.56 ..	11.2°
4.90 % tin.....	19.47 ..	14.4°
2.52 "	32.64 ..	17.1°
1.33 "	48.52 ..	16.8°
2.45 % silver.....	79.38 ..	19.7°
1.22 "	86.91 ..	20.7°
3.50 % gold.....	65.36 ..	18.7°
0.31 % antimony.....	64.5 ..	12.°
0.29 % lead.....	64.5 ..	12.°

These results are approximately the same as those we have obtained, and which we have already referred to.

A third table exhibits the influence of the presence of copper oxide in the metal.

The first five examples are of cold-drawn wire of pure copper:

No.	Mode of Preparation.	Conductivity.	Temperature.
1.	Reduced by hydrogen....	93.00 ..	18.6°
2.	Deposited by battery; not fused.....	93.46 ..	20.2°
3.	Commercial copper	93.02 ..	18.4°
4.	Ex. No 3 remelted in hydrogen.....	92.76 ..	19.3°
5.	The same treated with a jet of hydrogen while fused..... (It has been noticed that the conductivity has been raised about 2½ per cent. by annealing.)	92.99 ..	17.5°
	Copper containing an undetermined amount of oxide.	73.32 ..	19.5°
	Deposited copper taken from a bar, fused on charcoal, and cast in an atmosphere of hydrogen	93.3 ..	12.8°
	Copper same as preceding taken from a porous bar; cast in a mould under ordinary conditions.....	94.8 ..	13.°
	Deposited copper, treated to cementation with charcoal, and containing silicon with traces of iron and phosphorus.....	62.8 ..	13.°

It may be added as a commentary on the last results that in France pure copper wire is prepared by a special process, which completely eliminates the oxide. Copper thus prepared has the conducting power of silver. It has been measured recently by the telegraphic authorities mentioned above, and with the result of exhibiting a conductivity of 102 by the standard heretofore used, and whose imperfections were thus demonstrated.

I cannot remember by what chain of circumstances I was led to study the compounds of copper and silicon, prepared for the first time by M. Henri St. Clair Deville, and apply them practically to the uses of the telegraph and the telephone.

Coppers and bronzes prepared under such conditions are much used for aerial telegraph and telephone lines. Their electrical and physical qualities permit us to believe that the time is gone in which iron and steel enjoy the monopoly in such transmission.

It is certain, as has been remarked by the eminent electrician in chief of the London Post Office: "Aerial lines should no longer be limited to the single quality of tensile strength. At a time when the apparatus for rapid transmission occupies all the great telegraphic routes, the lines should be made of very good conductors; the surfaces reduced so as to offer the least possible chance for induction, and they ought also to offer but little surface to the wind and snow, and should be practically indestructible."

Mr. Preece does not hesitate to conclude that iron and steel wires have had their day, and they will soon be replaced by wires of copper and its alloys. The latter fulfill not only all the required con-

ditions, but when once erected, cost no more than lines heretofore used.

VARIATION OF CONDUCTIVITY WITH CHANGE OF TEMPERATURE.

The influence of temperature on electric conductivity is well known. Heat expands bodies, and thus separates their molecules by spaces of greater or less width.

It would seem to follow therefore that heat should modify the conducting power in a manner depending upon whether the molecule or the intervening space is the better conductor.

In the case of metals, elevation of temperature diminishes the conductivity. Silver sulphide, on the other hand, according to Faraday, gains in conducting power as its temperature is raised.

Non-metallic elements are unlike the elements in this respect. A familiar example is afforded by the incandescent lamps. The conductivity of the filament is notably greatest at the time of incandescence than when it is cold.

Becquerel made researches upon this subject in 1846. Without considering the details of his method, we will limit ourselves to mentioning the fact and to giving some of his results. The coefficient of increase of resistance is given in the following table, for each degree rise in temperature:

The coefficient is the value of k in the formula: $R=r(1+kt)$; in which r is the resistance at 0° and R that at $t°$.

Mercury	0.001040
Platinum	0.001861
Gold	0.003397
Zinc	0.003675
Silver	0 004022
Cadmium	0.004040
Copper	0 004097
Lead	0.004349
Iron	0 004726
Tin possibly containing lead.	0.005042
Tin, pure	0.006188

The change of volume due to the expansion has in reality no effect. The augmentation of conductivity, when this is the consequence, is nearly balanced by the diminution resulting from the increase in length, which is a part of this same expansion.

The relative conductivity of several metals (the bodies of most interest in this relation) are given in the following table, which should be compared with that by the same physicist previously given for the mean temperature of 12.75°:

	Conductivity at 0°.		at 100°.
Silver, pure, annealed	100.	..	71.316
Copper, pure, annealed	91.517	..	64.919
Gold, " "	64.960	..	48.489
Cadmium	24 579	..	17.506
Zinc	24.063	..	17.596
Tin	14.014	..	8.657
Iron, annealed	12.350	..	8.387
Lead	8.277	..	5.761
Platinum, annealed	7.933	..	6.688
Mercury, distilled	1.7387	..	1.5749

We have at present no similar experiments of our own with which to compare these. We may therefore accept these results for the present, with the reservation before expressed, relating to the purity of the metals and the precision of the methods. We shall have occasion to refer to this subject in the next communication.

ELECTRIC COMPARED WITH THERMIC CONDUCTIVITY.

The prevalent notions regarding the identity of divers physical phenomena have led physicists to seek to determine the relation between heat, light, and electricity.

Without repeating the theories bearing upon this subject, I may recall the opinions expressed in relation to the analogy between electric and thermic propagation, by giving the figures previously referred to, and specify the experiments, the results of which will form the subject of a later communication

The analogy referred to, has been supposed to exist since it was known that some metals appeared in the same order when classified either by their electric or their thermic properties. Also metals are good conductors of heat and electicity, while dialectrics are generally poor conductors of heat. Furthermore, the coefficients of conduction for heat and electricity are for many metals nearly the same.

The researches of Wiedmann and Franz yielded the following results:

	Conductivity. Thermic.		Electric.
Silver	100	..	100
Copper	74	..	73
Gold	53	..	59
Brass	24	..	22
Tin	15	..	23
Iron	12	..	13
Lead	9	..	11
Platinum	8	..	10
German silver	6	..	6
Bismuth	2	..	2

These results are certainly very interesting, and at first view tend to mislead those experimenters who seek to simplify the explanations of physical phenomena, and who are always seeking to unify the theoretical conceptions of elementary laws.

Meanwhile, we do not find it possible to accept unreservedly these results, nor the conclusions which it is desired should be drawn from them. We know that the table of electric conductivities needs revision. Copper, for example, appears in the table of Wiedmann and Franz with a coefficient of 73 for electric conductivity, whereas it is well known to be entitled to rank with silver at the top of the scale.

We find, also, some remarkable divergences in the following list:

	Conductivity. Thermic.	Electric.
Copper	73.6	100.
Gold	53.2	78.
Zinc	19.1	29.9
Aluminum	19.6	54.2
Silicious bronze	68.	98.
Antimony	21.5	3.88
Tin with sodium	13.34	46.9

The whole question clearly deserves to be restudied without the embarrassments of preconceived theories.

We have undertaken this new study, and we will limit ourselves here to describing our preliminary experiments. These researches have been carried on with the valuable assistance of M. Jannetaz; *Maitre de Conférences* of the Sorbonne, who has willingly brought to our aid his profound knowledge of the subject, and the apparatus with which he prosecuted his previous labors in determining the conduction of heat in minerals.

The process which he employed and which we have adopted is as follows:

The apparatus as devised by M. Jannetaz, consists essentially of a plate upon which is placed the mineral, whose conducting power is to be tested. This body has been previously prepared by coating a plane face of it with a thin layer of wax or grease. In contact with this is a small bead of platinum about the size of the head of a pin, which has been welded to the apex of a triangle of platinum wire. A current being sent through the wire heats it; the bead fusing the wax over a circular or elliptical space, whose dimensions, taking into account the temperature of the wire and the time of contact, afford data for determining the conducting power of the specimen.

To complete the description, it is only necessary to add that the wire is brought through a copper casing which is supplied with a current of cold water, and that the wire is also supported by a carrier, which facilities making contact with or withdrawal from the specimen.

It is with this apparatus that we have begun our researches. Unfortunately, it has not been possible in the time available, to prepare thin plates of the metals and alloys. We have been obliged to content ourselves with half-cylinder sections, cut from the little bars employed in the previously mentioned experiments. Under these circumstances, the external conditions may have modified the results, and we prefer at present not to exhibit numerical results which may require correction.

It is sufficient now to say that some of the results appear to invalidate the law of identity above referred to, and others lead to the interesting conclusion that there may be a difference of conductivity in the same metal depending upon the method of preparation, whether it was cast, drawn, or rolled.

You will notice upon the specimens submitted to you, which bear traces of these experiments, that the elliptical form of the curved spaces takes a direction corresponding to the lamination of the specimen.

We have completed the observations which we set out to present to you, and which, as you see, form a programme of researches to be made quite as much as of results accomplished.

We wish especially before closing, to call your attention to the double interest attaching to the subject which has enlisted your attention. From the practical point of view alone, the subject is of the highest importance, as has been shown by the results obtained, bearing as they do upon the vast problem of transmission of electric energy.

But we do not insist upon this point. The estimate of this subject from the scientific standpoint is no less great, involving as it does the nature of electricity itself.

Among the facts which we have noted, we will refer again to the strange anoma-

ly exhibited by the alloys as conductors of electricity, when we consider them as compound substances. This might serve as an occasion to review the theories promulgated by certain eminent physicists; Clerk-Maxwell among others. But these theories are yet such mere hypotheses, and their interpretation appear to me so abstract that I fear to venture upon this ground.

But there is no rashness in affirming that in the resistance offered by a body to the electric current, the magnetic orientation of the molecules of each body plays an important part. Each of them, influenced by the passage of the current to a degree probably depending upon its constitution, modifies the resistance.

The question may be asked whether the compound molecules of the alloys do not behave as other elementary electric couples, having a special electromotive force that determines the passage of the current, and which produces in the total mass a series of currents analogous to the so-called Foucault currents, the effect of which is contrary to that of the primary current.

We are tempted to believe that in the alloys the chains of molecules of the same kind are not able to act like a cable made of wires of different metals. As the alloy of equal parts of gold and silver has already been referred to, let us determine the resistances under the different conditions. If the current be transmitted through two wires each a kilometer long and a millimeter in diameter, one being of gold and one of silver, the formula for derived currents enables us to determine the total resistance. The formula is:

$$x=\frac{R_1 R_2}{R_1+R_2}$$

The resistance of the silver being 19 ohms, and of the gold 24.36 ohms, the total is by the above formula, 13.38 ohms.

But a wire made of the alloy in these proportions, having the same length and a double cross-section, offers a resistance of 58.50 ohms.

This result shows that the law governing derived currents does not apply in the case of conduction through alloys.

Some other cause intervenes; possibly such an one as is mentioned above; perhaps an unknown cause which essentially modifies the flow of electricity.

We may be permitted to recommend this investigation to physicists, and we are ready to devote our efforts to the same end.

If this task, to the accomplishment of which, we wish to dedicate ourselves, is crowned with success, we shall be sufficiently recompensed by the satisfaction which labor affords to those who seek thereby to advance science.

SECONDARY BATTERIES.*

By FRANK GEERE HOWARD, Stud. Inst. C. E.

From Selected Papers of the Institution of Civil Engineers.

Among the most important discoveries of recent years few have received so much attention, and had such bright prospects, as the accumulators of Planté and their various modifications. All sorts of wild schemes were set afloat for their utilization, the principal one being to talk about the time when electricity by means of these accumulators could be delivered every morning from house to house like milk. The result of this would have been much the same as if a water company attempted to supply water to its customers by sending round so many gallons every day in carts, or better still a gas company delivering bags full of gas ready for use, one being as impracticable as the other. These ideas were soon dispelled, and then followed disappointment, and of course general condemnation of secondary batteries.

But all this time inventors had been steadily turning their attention in this direction. Many improvements have been introduced, and numbers of difficulties, before considered fatal to them, have been

* This communication was read and discussed at a meeting of the students on the 16th of January, 1885.

overcome. They are still far from perfect, though it may yet be expected that accumulators or secondary batteries will play the most important part in all extended systems of electric lighting, transmission of power, and the satisfactory solution of the utilization of wind and water power.

The earliest form of accumulator was a voltameter worked backwards, and was first observed in 1801, by the French chemist, Gautherot. Two years later, Ritter, in Germany, carried out a number of experiments in the same direction, and endeavored to utilize the reaction; he employed for this purpose two plates of gold, separated from each other by flannel kept moistened with acid, and charged by an ordinary Volta's pile. If two pieces of platinum be immersed in dilute sulphuric acid and connected to the two poles of a battery, so that a current be sent through them, the liquid will be decomposed, hydrogen being driven off at one plate and oxygen at the other. If, then, the battery is disconnected and the plates circuited through a galvanometer, it will be seen that they give out a current in the opposite direction to the charging current.

Becquerel first pointed out the real cause of the returned current, upsetting the original theory that the plates absorbed the current during the charging, and gave it up again during the discharge. He showed that the returned current was not due to the storage of electricity, but to the presence of substances having chemical affinities for each other, derived from the decomposition going on during the charge.

If two sheets of ordinary lead be placed in dilute sulphuric acid, and a current be sent through them, a dark brown deposit will form upon the plate connected to the positive pole of the charging battery, due to the formation of peroxide of lead on the surface of that plate, whilst the other will be reduced slightly to spongy lead. Upon disconnecting these plates from the charging source, and circuiting them through a galvanometer, a current will flow from the brown or peroxidized plate to the other reduced plate.

The result is the reduction of the peroxide of lead on the one plate to oxide, and then to sulphate of lead from the presence of the sulphuric acid in the solution; and of the spongy lead to sulphate of lead. Upon recharging, the anode or brown plate, attached to the positive pole of the charging source, will again become peroxidized, and the cathode or other plate reduced to spongy lead. The sulphate of lead will thus disappear from both plates. The plates are thus continually oxidized and deoxidized as they are charged and discharged.

This is a second battery in its simplest form, and all accumulators are based upon this principle. Lead has proved by far the best metal for the purpose, because it forms an almost insoluble oxide; a few other metals are nearly insoluble, but lead has the advantage. It is better than silver or manganese for the reason that its oxide is less soluble than either of them.

The initial electromotive force of a freshly prepared cell when charged is 2.25 volts, but this drops after the first few minutes of the discharge to a little less than 2 volts, which may be taken as the normal electromotive force of a cell made up of peroxide of lead, spongy lead and dilute sulphuric acid. The constancy of the electromotive force of a cell varies considerably with the rate of discharge. If it is run down, or discharged, at a very rapid rate it falls off very quickly; whereas, if only a small current be taken out, the electromotive force will remain constant for a long time, though the loss on the negative plate or anode from local action will be greater. The strength of the electrolyte or solution varies considerably during the charging or discharging of the cell. When the cell is fully charged the solution is strongest, and when discharged weakest. From this it follows that the internal resistance of a battery is lowest when the electromotive force is at its highest, and the converse. Dr. Frankland has endeavored to utilize this change in the specific gravity of the solution, to indicate the condition of the cell, and has found that a change of 0.005 in the specific gravity was equal to a storage of 20 ampere hours. This method of determining the state of the cell does not in practice give very good results, on account of the difficulty of getting the solution in the cell to circulate freely, so as to be of the same density throughout.

The first practicable form of accumulator was that of Planté, made in 1860. He took two sheets of lead, and placing them

in dilute sulphuric acid, passed a current through them for some time. Then disconnecting the cell from the charging source, he discharged it and again charged it, but this time in a reverse direction; so that the lead plate, formerly connected to the positive pole of the charging source, was this time connected to the negative pole. A number of charges and discharges were thus alternately effected until the plates were rendered sufficiently porous to give a satisfactory storage capacity. Planté found that it was necessary to continue this process for some months before a battery was fit for use. Of course when the battery was used the current was sent through it always in the same direction. The reverse charges were only employed to "form" the plates, or render them sufficiently porous to hold a charge. Unfortunately, this form of cell, simple as it is, does not store much electrical energy, except when very thin plates are employed, and these soon become brittle and liable to fall to pieces through being entirely converted into peroxide of lead and thereby losing their stability.

According to Géraldy, a Planté cell containing 1.445 kilogram of lead, is capable storing 4,983 kilogrammeters of energy, or 11,329 foot-pounds per lb. of lead. De Meritens modified the Planté cell by making the plates out of a number of thin sheets of lead laid one on top of the other, and soldering their edges together, thus obtaining plates composed of a number of layers of thin sheet lead.

Kabath again modified this by taking long thin strips of lead which he corrugated by passing them through grooved rollers, and then interleaving them with plain strips, or ribbons of lead, folding them backwards and forwards, until a plate was formed of the required size. The drawback to this form is that the lead used is of necessity very thin, and thus soon breaks down and renders the cell useless. In both these cases the same tedious process of forming as in the Planté cell has to be gone through.

In 1880, Faure devised the plan of pasting red lead, or minium, upon plates of plain lead; this paste was rapidly peroxidized and reduced in one charge, thus overcoming and doing away with the long process of forming the Planté plates. The paste had to be held on the plates by wrapping them in flannel or some such porous material. This increased the internal resistance of the cell, and after a time the flannel began to get rotten and fall off, causing the cell to fail. A much higher storage capacity, however, was obtained, it being 18,000 foot-pounds per lb. of lead and minium. Many improvements have been introduced by Messrs. Sellon and Volckmar, culminating in the form now manufactured by the Electrical Power and Storage Company, which consists of grids of cast-lead with holes from $\frac{1}{4}$ to $\frac{3}{8}$ inch square.

The grid used for the anode is made thicker than that for the cathode. These castings are filled with a paste of minium and dilute sulphuric acid. After being dried they are placed in long troughs, filled with dilute sulphuric acid, and a current is sent through them to form them. The amount of current necessary to convert 1 lb. of minium to peroxide is 70 ampere hours, and 140 ampere hours to reduce it to metallic lead. But as it only needs 110 ampere hours to reduce 1 lb. of litharge, this is generally employed for the cathode plate. After the current has been sent through the plates, they are taken out, washed, and stacked until required for use, when they are mounted in their respective cells, and employed for lighting or other purposes. No felt is put between these plates; they are separated from each other by india-rubber bands, pieces of wood, or some similar device.

The storage capacity of this cell is considerably higher than that of the Faure type, being 48,000 foot-pounds per lb. of lead. But since the storage capacity of a theoretically perfect secondary battery is 360,000 foot-pounds per pound of lead, even the best cell falls far short of this. The figures in Table I., against Sellon-Volckmar, are the results obtained from cells specially constructed for driving electric launches, tramcars, &c. Those employed ordinarily for lighting purposes yield about 36,000 foot-pounds per lb. of lead.

It has been proposed by Mr. Fitzgerald, to substitute carbon for the lead employed for establishing contact with the active material of the cathodes, where oxides of lead are used, and also to protect parts of the anodes by means of some insulating material, so as to arrest the process

TABLE I.—STORAGE CAPACITY OF THE VARIOUS SECONDARY BATTERIES AS REGARDS RATIO OF WORK TO WEIGHT.

NAME.	Storage Capacity in foot-lbs. per lb. of Lead.	Percentage of Efficiency.
Theoretically perfect cell.	360,000	100.00
Sellon-Volckmar.....	48,000	13.13
Crompton-Fitzgerald	24,000	6.66
Faure	18,000	4.99
Planté.....	12,000	3.33

of peroxidation over the whole of the plates, and thus always to preserve a good contact throughout the anode. A large number of experiments have been carried out in this direction. The best results obtained were from a battery made up of cathodes composed of minium and carbon fragments, held in a punctured lead envelope, and anodes composed of spongy or finely-divided lead, also held in a punctured lead envelope, and protected by a network of insulating material, generally known under the name of Prout's glue. This form had a storage capacity of 24,000 foot-pounds per lb. of lead. Messrs. Beaumont and Biggs' battery is made up of plates of compressed spongy lead. The lead is obtained in this state by precipitation from a solution of acetate, or sugar of lead, in which solution is placed a plate of zinc. Lead in a state of very fine subdivision is deposited upon the zinc, from which it is scraped off, put into moulds, and subjected to hydraulic pressure. By this means plates of great porosity are obtained, which should have as high a storage capacity as any known form of battery. Of the numerous batteries in the market, nearly all are modifications of the well-known Planté type, which seems to be the best form for durablity, if not for storage capacity; and the "life" of a battery is of far more importance than its storage capacity per lb. weight. The most notable battery of this class, and one coming prominently into notice, is manufactured by the Wolverhampton Electric Light Co., called the Elwell-Parker accumulator. The plates of this battery are made of sheet-lead only, but before being formed they are immersed in a strong solution of nitric acid, which attacks the surface of the lead, and honeycombs it to a certain extent. Thus, according to Planté, who first patented this process in 1881, the long process of forming is to a great extent done away with. There seems, however to be considerable doubt whether this is the case, or if the nitric acid simply dissolves the lead. In all probability the acid attacks the impurities in the lead, and thus prepares the plate in the best possible manner to receive the current. Another battery is the B. T. K., so called from the names of the inventors, Messrs. Beeman, Taylor, and King. This is a modification of the Kabath accumulator, consisting of plates made of alternate corrugated and plain strips of lead, wound round and round upon themselves. Batteries of this description are being largely used in the district lighting at Colchester.

An improvement by Dr. Frankland is claimed upon the Sellon-Volkmar type of battery, by hardening the paste of minium, or other oxide of lead used in the plates, by immersing the plates in solutions of sulphuric acid of various densities during different periods. He also claims for the plates the capacity of receiving a higher rate of charge. This hardening mixture may be cast into plates or cylinders of any desired form or size. There are also many other batteries of the Planté type by various inventors. Mr. Joel has patented a plate having a considerable storage capacity. He makes the plates of lead-wool, or perhaps more properly speaking, lead-fiber, mixed with minium, and then pressed upon a cast-lead plate, which acts both as a conductor and support.

A battery differing in many ways from those already mentioned, was that proposed by Sutton. He took two plates, one of lead and the other of copper, and immersed them in a solution of sulphate of copper. The lead plate became peroxidized, and the copper was alternately dissolved and deposited from the solution of sulphate of copper. This battery, though giving good results upon a small scale failed when used to a large extent. The electromotive force is much lower than that of a cell composed of lead only.

The advantages of accumulators made after the Planté type over those of the Faure type are that they afford a far higher rate of discharge. This property is invaluable for many purposes, as, for

instance, where numerous lamps are required to be run for only a short time. But then, again, accumulators of the Faure type take in a higher rate of charge than the Planté, because they have a greater thickness of working material, and also store a large amount of electrical energy in a given weight.

The durability, or life, of a secondary battery, has never been satisfactorily settled. Manufacturers assert that their accumulators will last for a number of years, but this has not yet been proved. Those of the Sellon-Volkmar type cannot, the author believes, be depended upon to last more than five or six months at the outside, when they are worked regularly every day up to their full capacity. But when overcharged continuously, or otherwise improperly used, a few days will often suffice to put them out of order.

The Planté accumulator should last longer than this, but it must be borne in mind that when this battery has arrived at its maximum of efficiency, it is on the point of falling to pieces. Where water or steam power is available, a rough but serviceable battery may be made by taking sheets of thick lead, cutting them into plates of any required size, and mounting them in tanks filled with dilute sulphuric acid. Of course, during the first few charges the storage capacity will be very small, but this will improve every day; at the same time the cost of forming them is next to nothing. Eventually it will prove to be a most efficient battery.

The first and most important point in setting up a secondary battery is to carefully insulate the cells from each other, and from any moisture. This is best effected by arranging them on wooden shelves raised about 8 inches off the floor, and at an equal distance from the walls. The boxes should be left with a space of at least an inch between them, and, wherever possible, placed in single rows, and not upon shelves one on top of the other, so that free access may be had to each box. In large installations the cells are often of considerable size, weighing from 5 to 6 cwt. each, thus rendering it extremely difficult to move them. For these reasons, they should be so arranged that, once put up, they can be easily got at for repair without being removed. The importance of keeping the cells perfectly free from moisture can hardly be overrated. If they should be placed upon a damp floor, or in any position where moisture can get to them, there will be a considerable leakage of electricity, the cells will rapidly run down (*i. e.*, become discharged), and will be in all probability condemned as unworkable and useless.

All secondary batteries, when first set up, should be tested for insulation. This may easily be done by connecting one pole of the battery (after it has been charged) to one terminal of an ordinary galvanometer, and the other terminal of the galvanometer to earth. The best way to obtain a good earth is to connect the wire to a water or gas pipe; or, if neither of these should be available, to drive a piece of iron into the ground and make a connection in that way. If the needle is deflected it indicates leakage, which should at once be remedied. The leakage will in all probability be found to arise from an escape of the solution from one of the cells, the place where they are standing being damp, or one of the leads "making earth." The poles of the cells should be connected together by stout bars of metal. It is inadvisable to use copper on the positive pole of a cell, on account of the rapidity with which it corrodes. Plain lead connections, or a mixture of lead and antimony, are the most trustworthy. The positive pole of one cell is joined to the negative of the next, and so on all through the series. Then the positive pole of the dynamo is connected to the last positive of the battery, and the negative to the negative. The contacts must be properly made between each cell, as a bad contact will produce a great deal of heat in the cells, thereby introducing resistance into the circuit, and reducing materially the rate of charge or discharge. Care should be taken not to charge the battery at too rapid a rate, and if at any time during the charging the cells become warm, the charging should be stopped, and the battery allowed to cool down. The heating of the plates causes them to buckle, often resulting in the short circuiting of the cell, and thus putting it out of use until the defect has been remedied.

During the charging, and also whilst they are at rest, it is advisable to test

the cells from time to time. This may be readily done by taking a short length of stout wire, holding one end upon one pole of the cell to be tested, and striking the opposite pole with the other end of the wire. If the cell is in good condition it should give a bright, crisp spark. If it gives no spark at all it shows that there is a short circuit in the cell, arising, in all likelihood, from two of the plates touching each other, or from a small piece of some conducting substance having fallen down between the plates. This should be at once remedied, otherwise the cell should be cut out from the circuit and taken to pieces. It is very important, when working batteries, to keep all the cells in as even a condition as possible. For, if a cell be empty, or is discharged before the others, it will then become recharged in the reverse direction, and thus set up an opposing electro-motive force to the rest of the battery, besides there is the risk of spoiling that particular cell. Keeping all the cells in the same condition is best effected by observing them when charging, and cutting out of the circuit all those cells that give off gas freely, and continuing to charge the others until they are all fully charged. If this is done about once in two months, the working efficiency of the battery will be maintained at a very high point. Of course, this entails extra labor, but the author contends that secondary batteries do, and always will, require skilled supervision. Care must be taken not to cut out too many cells whilst charging the battery without introducing a corresponding resistance, or the dynamo will be burnt.

The ordinary strength of the electrolyte is 1 part of sulphuric acid to 9 parts of water, or of a specific gravity of 1.2, that of sulphuric acid being 1.84. The water should be as pure as possible. When working a battery it is necessary to keep all the plates immersed in the electrolyte. If it becomes necessary to add to the electrolyte, to keep the cell full, water only should be used, otherwise the solution will become too strong. When batteries are left out of use for any length of time, it is advisable either to remove the solution and wash the plates and boxes with water, or else to leave them fully charged. This latter method is perhaps best adopted only when the battery is not to be out of use for very long. It is found that, when batteries are left idle and full of the solution, sulphate of lead will form all over the plates, and have a serious effect upon their future working.

By discharging a cell too rapidly, as much harm can be done to it as by charging it too rapidly.

The best dynamos for charging accumulators are shunt dynamos. Both the series and compound machines are liable to have their poles reversed by the batteries discharging through them, which is likely to arise from the speed of the dynamo decreasing, and thereby having its electromotive force overpowed by that of the battery. This difficulty might be overcome by specially-designed automatic switches. The most economical way is to charge each battery in two series in parallel, though, of course, while charging them in this manner, no lights can be run at the same time. The circuit should be so arranged that the dynamo is charging the battery in two series in parallel, or in one series with the lamps in parallel, or dynamo, lamps and battery all in parallel. In order to charge an accumulator, it is necessary to employ a dynamo giving an electromotive force greater than that of the cells, and greater in proportion to the rate of charge required; but as all excess of electromotive force is lost energy, a slow charge is the most economical where time is of little moment.

The uses to which accumulators may be put are almost endless. They are serviceable for every application of the electric current. Lighting being at present the most in need of them, by employing an accumulator in any installation, the failure of the light should be rendered almost impossible. The chief causes of break-downs are the slipping or breaking of belts, the heating of the bearings, or some trivial mishap with the engine which may be set right in a few minutes, but which is sufficient to plunge the place so lighted into darkness. Now, by employing accumulators, if the engine has to be stopped, the batteries immediately come into action, and should run the lights long enough for the necessary repairs to be effected. In nine cases out of ten the failure of the electric light can be traced to some minor breakage or mishap

that can be remedied in half an hour, or even less; but, of however short duration, a total extinction of the lights must follow when working direct from the dynamo. Again, in almost all factories and workshops where steam power is used, there is a surplus of from 2 to 4 HP. This surplus, if employed to drive a small dynamo and to charge batteries all day, would be sufficient to run enough incandescent lamps to light up the whole of the shops in the evening. Accumulators may also be used advantageously in steadying the light when the dynamo is worked by a gas-engine, or any irregular source of power. The only method of satisfactorily solving the problem of the utilization of water power is by employing storage batteries, so that the water, instead of running to waste day and night, could be made to store electricity, which, in its turn, could either be used for motive power or for ordinary lighting purposes, and be drawn off at will. This applies also to the possibility of utilizing wind power.

The employment of secondary batteries for driving trains, tramcars, boats, &c., has been a good deal discussed, but up to the present time, although many costly experiments have been undertaken, no practical work has been done.

The first experimental launch worked by accumulators was that fitted up by the Electrical Power Storage Co., and launched at Millwall in September, 1882. She was 25 feet long, 5 feet beam, with accommodation for about ten passengers. She was driven by two Siemens dynamos of the $S D_2$ type, coupled in parallel. The current was supplied by forty-five 1 HP. Faure Sellon-Volkmar cells. The two motors running together absorbed a current of 46 amperes, and developed abont 3.75 HP. The weight of each motor was 316 lbs., that of the countershaft, &c., was 180 lbs., and the total weight of the accumulators was 2,520 lbs., or 56 lbs. per cell.

The second launch was built by Messrs. Yarrow & Co., of Poplar, and was fitted with all the electrical arrangements by the Electrical Power Storage Co., for the Vienna Exhibition of 1883. She is 40 feet long, 5 feet beam, and will carry about forty passengers. In this boat the motor and batteries are placed under the floor. The motor used is a Siemens D_2 dynamo, which, with eighty Faure-Sellon-Volkmar accumulators (the number carried), will develop 7 HP. Its weight is 658 lbs., and the efficiency, when tested, was 78 per cent. In this boat no gearing is used between the motor and screw-shaft, the former being coupled directly to the latter. The average speed attained was 8 miles per hour. The advantages are, that whereas two men are necessary to work a steam launch, the electric launch needs but one man to look after it, as he can steer, stop, or regulate the speed at the same time. An electric launch will also carry more passengers, besides being free from smoke, smell, heat and noise.

The experiments carried out with tramcars have not yet advanced nearly so far. A trial trip was made on the Gunnersbury and Kew Tramway line on the 10th of March, 1883. The experimental car, as in the case of the launch, was fitted up with the Electrical Power Storage Co., and was of the ordinary type used upon the London tramways. It was driven by a Siemens dynamo, designed to work with an electromotive force of 100 volts, and a current of 60 amperes furnished by 50 accumulators placed under the seats of the car. The cells measured 13 inches by 11 inches by 7 inches, and weighed 80 lbs., their total weight being 4,000 lbs. The storage capacity of each cell was 560 ampere-hours. Apart from this experiment, which id not altogether end successfully, the problem of propelling tramcars has not been solved. The same company exhibited a tricycle at the Vienna Exhibition, worked by twenty-one small accumulators and a motor designed by Mr. Reckenzaun, of London. This would run for about one hour, and cover about 8 miles in that time. The main reason why electricity has not yet been employed for motive power for boats and tramcars is, the author thinks, undoubtedly to be traced to the still imperfect and unsatisfactory state of secondary batteries. When once they have been so far improved as to be looked upon as trustworthy, a great and lasting impetus will certainly be given to the utilization of electricity for motive power as well as for lighting purposes.

THE WATER SUPPLY OF ANCIENT ROMAN CITIES.*

By Prof. W. H. CORFIELD, M.D., M.A.

From "The Building News."

As the supply of water to large population is one of the most important subjects in connection with sanitary matters, and one upon which the health of the populations to a very large extent depends, I propose to give a short account of some of the more important works carried out for this purpose by the ancient Romans —the great sanitary engineers of antiquity —more especially as I have had exceptional opportunities of examining many of those great works in Italy, in France, and along the north coast of Africa. Of the aqueducts constructed for the supply or Rome itself, we have an excellent detailed account in the work of Frontinus, who was the controller of the aqueducts under the Emperor Nerva, and who wrote his admirable work on them about A. D. 97. It may be interesting in passing to mention that Frontinus was a patrician, who had commanded with distinction in Britain under the Emperor Vespasian, before he was appointed by the Emperor Nerva as controller (or, we should say, surveyor) of the aqueducts. He was also an antiquarian, and in his work he not only describes the aqueducts as they were in his time, but also gives a very interesting history of them. He begins by telling us that for 441 years before the building of the city—that is to say, B. C. 312—there was no systematic supply of water to the city; that the water was got direct from the Tiber, from shallow wells, and from natural springs; but that these sources were found no longer to be sufficient, and the construction of the first aqueduct was undertaken during the consulship of Appius Claudius Crassus, from whom it took the name of the Appian aqueduct. This was, as may be expected from its being the first aqueduct, not a very long one; the source was about eight miles to the east of Rome, and the length of the aqueduct itself rather more than eleven miles, according to Mr. James Parker, to whose paper on the "Water Supply of Ancient Rome" I am indebted for many of the facts concerning the aqueducts of Rome itself. This aqueduct was carried underground throughout its whole length, winding round the heads of the valleys in its course, and not crossing them supported on arches, after the manner of more recent constructions; it was thus invisible until it got inside the city itself, a very important matter when we consider how liable Rome was, in those early times to hostile attacks. It was soon found that more water was required than was brought by this aqueduct, and it was no doubt considered desirable to have tanks at a higher level in the city than those supplied by the Appian aqueduct. It was determined, therefore, to bring water from a greater height, and from a greater distance, and the river Anio, above the falls at Tivoli, was selected for this purpose. The second aqueduct, the Anio Vetus, was no less than 42 miles in length, and was, like the Appian, entirely under the surface of the ground, except at its entrance into Rome, at a point about 60 ft. higher than the level of the Appian aqueduct. Little search has been made for the remains of this aqueduct, and its exact course is not known; but during my examination of the remains of the subsequent aqueducts at a place the Porta Furba, near Rome, where the ruins of five aqueducts are seen together, and at, or close to, which point the Anio Vetus must also have passed underground, I was rewarded for my search by discovering a hole, something like a fox's hole, leading into the ground, and on clearing away a few loose stones which had apparently been thrown into it, and putting my arm in I found that it led to the specus or channel of an underground aqueduct, and on relating this incident to the late Mr. John Henry Parker, the antiquarian, who was then in Rome, and showing him a sketch of the place, he said that he had no doubt that I had been fortunate enough to discover the exact position of the veritable Anio Vetus at that spot. These two aqueducts

* An address delivered before the Sanitary Institute of Great Britain.

sufficed for the supply of Rome with water for about 120 years, for Frontinus tells us that 127 years after the date at which the construction of the Anio Vetus was undertaken—that is to say, the 608th year after the foundation of the city—the increase of the city necessitated a more ample supply of water, and it was determined to bring it from a still greater distance. It was no longer considered necessary to conceal the aqueduct underground during the whole of its course, and so it was in part carried above ground on embankments, or supported upon arches of masonry. The water was brought from some pools in one of the valleys on the eastern side of the Anio, some miles further up than the point from which the Anio Vetus was supplied, and the new aqueduct, which was 54 miles in length, was called the Marcian, after the Prætor Marcius, to whom the work was intrusted. Frontinus also tells us the history of the other six aqueducts which were in existence in his time, viz.: the Tepulan, the Julian, the Virgo, the Alsietine or Augustan, the Claudian, and the Anio Novus, the last two being commenced by the Emperor Caligula, and finished by Claudius, because "seven aqueducts seemed scarcely sufficient for public purposes and private amusements"; but it is not necessary for our purpose to give any detailed account of the course of these aqueducts, it is only necessary to mention one or two very interesting points in connection with them. In order to allow of the deposit of suspended matters, piscinæ, or settling reservoirs, were constructed in a very ingenious manner. Each had four compartments, two upper and two lower; the water was conducted into one of the upper compartments, and from this passed, probably, by what we should call a standing waste or overflow pipe, into the one below; from this it passed (probably through a grating) into the third compartment at the same level, and thence rose through a hole in the roof of this compartment into the fourth, which was above it, and in which the water, of course, attained the same level as in the first compartment, thence passing on along the aqueduct, having deposited a good deal of its suspended matter in the two lower compartments of the piscinæ. Arrangements were made by which these two lower compartments should be cleaned out from time to time. The specus, or channel, itself was, of course, constructed of masonry, generally of blocks of stone cemented together, and it was frequently, though not, it would appear, always, lined with cement inside. It was roofed over, and ventilating shafts were constructed at intervals; in order to encourage the aeration of the water, irregularities were occasionally introduced in the bed of the channel. The water supplied by the different aqueducts was of various qualities; thus, for instance, that of the Alsietine, which was taken from a lake about 18 miles from Rome, was of an inferior quality, and was chiefly used to supply a large naumachia, or reservoir, in which imitation sea fights were performed; while, on the other hand, the water of the Marcian was very clear and good, and was therefore used for domestic purposes. Frontinus gives the most accurate details as to the measurements of the amount of water supplied by the various aqueducts, and the quantities used for different purposes. From these details Mr. Parker computes the sectional area of the water at about 120 square feet, and says: "We can form some opinion of the vast quantity if we picture to ourselves a stream 20 ft. wide, by 6 ft. deep, constantly pouring into Rome at a fall 6 times as rapid as that of the River Thames." He considers that the amount was equivalent to about 332 million gallons a day, or 332 gallons per head per day, assuming the population of the city to be a million. When we consider that we in London have only 30 gallons a head daily, and that many other towns have less, we get some idea of the profusion with which water was supplied to ancient Rome. But the remains of Roman aqueducts are not only to be found near Rome. Almost every Roman city, whether in Italy or in the south of France, or along the north coast of Africa, can show the remains of its aqueduct, and almost the only things that are to be seen on the site of Carthage are the remains of the Roman water tanks and the ruins of the aqueducts which supplied them. The most beautiful aqueduct bridge in the world, on the course of the aqueduct which supplied the ancient Nemaucus, now Nismes, still stands, and is

called, from the name of the department in which it is, the Pont du Gard. It consists of a row of large arches crossing the valley over which the water had to be carried, surmounted by a series of smaller arches, and these again by a series of still smaller ones, carrying the specus of the aqueduct. This splendid bridge still stands perfect, so that one can walk through the channel along which the water flowed, and it might be again used for its original purpose. There was, however, one city which, from the fact that a great part of it was situated upon a hill, was more difficult to supply with water than any of the rest, and which, at the same time, from its size, its great importance, and the fact that it was the favorite summer residence of several of the Roman emperors, and notably of Claudius, who was born there, and who had a palace on the top of the hill, must, of necessity be supplied with plenty of water, and that, too, from a considerable height. I refer to Lugdunum (now Lyons), then the capital of Southern Gaul. This city was built by Lucius Munatius Plancus, by order of the Senate, in A. U. C. 711. Augustus went there in A. U. C. 738, and afterwards lived there from 741 to 744. It was he who raised it to a very high rank among Roman cities. It had its Forum near the top of the hill now called Fourvières (probably a corruption of Forum Vetus), an Imperial palace on the summit of the same hill, public baths, an amphitheatre, a circus, and temples. In order to supply this city with water, standing as it did on the side of a hill at the junction of two great rivers (now Rhone and Saone), it was necessary to search for a source at a sufficient height, and this Plancus found in the hills of Mont d'Or, near Lyons, where a plentiful supply of water was found at a sufficient height, viz., that of nearly 2,000 ft. above the sea. From this point an aqueduct, sometimes called, from its source, the aqueduct of Mont d'Or, and sometimes the aqueduct of Ecully, from the name of a large plain which it crossed, was constructed, or, rather, two subterranean aqueducts were made and joined together into one, which crossed the plain of Ecully in a straight line, still underground; but the ground around Lyons was not like the Campagna, near Rome, and it was necessary to cross the broad and deep valley now called La Grange Blanche. This, however, did not daunt the Roman engineers; making the aqueduct end in a reservoir in one side of the valley, they carried the water down into the valley, probably by means of lead pipes, in the manner which will be described more at length further on, across the stream at the bottom of the valley by means of an aqueduct bridge 650 ft. long 75 ft. high, and 28½ feet broad, and up the other side into another reservoir, from which the aqueduct was continued, along the top of a long series of arches to the reservoir in the city, after a course of about ten miles. In the time of Augustus, however, it was found that the water brought by this aqueduct was not sufficient, especially in summer, and as there was a large Roman camp which also requred to be supplied with water, situated at a short distance from the city, it was determined to construct a second aqueduct. For this purpose the springs at the head of a small river, called now the Brevenne, were tapped, and conveyed by means of an underground aqueduct (known as the aqueduct of Brevenne) which wound around the heads of the valleys, and, after a course of about 30 miles, is believed by some to have arrived at the city, but by others to have stopped at the Roman camp, and to have been constructed exclusively for its supply. I have here a diagram, after Flacheron, showing a section of this aqueduct, and this will give a very good general idea of the section of a Roman aqueduct where constructed underground. It will be seen that the specus, or channel, is 60 centimeters (or nearly 2 ft.) wide, and 1 m. 57 c. (or a little over 5 ft.) high, and that it is lined with a layer of 3 c. (or nearly 1¼ in.) of cement. It is constructed of quadrangular blocks of stone cemented together, and has an arched stone roof. It will be noticed also that the angles at the lower part of the channel are filled up with cement; it appears, also, that this aqueduct crossed a small valley by means of inverted siphons. But neither of these aqueducts came from a source sufficiently high to supply the Imperial palace on the top of Fourvières. Their sources are, in fact, according to Flacheron, at a height of nearly 50 ft. below the summit of Fourvières,

and it was, therefore, considered necessary by the Emperor Claudius to construct a third aqueduct. The sources of the stream now called the Gier, at the foot of Mont Pila, about a mile and a-half above St. Chamond, were chosen for this purpose, and from this point to the summit of Fourvières was constructed by far the most remarkable aqueduct of ancient times, an engineering work which, as will be seen from the following description—partly taken from Montfalcon's history of Lyons, partly from Flacheron's account of this aqueduct, and partly from my own observations on the spot—reflects the greatest possible credit on the Roman engineers, and shows that they were not, as has been frequently supposed by those who have only examined aqueducts at Rome, by any means ignorant of the elementary principles of hydraulics. To tap the sources of a river at a point over 50 miles from the city, and to bring the water across a most irregular country, crossing ten or twelve valleys, one being over 300 ft. deep and about two-thirds of a mile in width, was no easy task; but that it was performed, the remains of the aqueduct at various parts of its course show clearly enough. It commences, as I have said, about a mile and a-half from the present St. Chamond, a town on the river Gier, about 16 miles from St. Etienne. Here a dam appears to have been constructed across the bed of the river, forming a lake, from which the water entered the channel of the aqueduct, which passed along underground until it came to a small stream, which it crossed by a bridge, long since destroyed. After this it again became subterraneous for a time, and then crossed another stream on a bridge of nine arches, the ruins of some of the columns of which are still to be seen, and from these ruins it would appear that the bridge had, at some time or another, been destroyed, probably by the stream running under it having become torrential, and subsequently rebuilt; again it became concealed underground, to reappear in crossing a small valley and another small stream, when it was again concealed by the ground, and in one or two places channels were even cut for it through the solid rock, after which it reappeared on the surface at a point where now stands the village of Terre-Noire, and where it was necessary that it should, somehow or another, cross a broad and deep valley. It ended in a stone reservoir, from which eight lead pipes descending into the valley were carried across the stream at the bottom on an aqueduct bridge, about 25 ft. wide, and supported by twelve or thirteen arches, and then mounted the other side of the valley into another reservoir, of which scarcely any remains are now seen, from which the aqueduct started again, disappearing almost immediately under the surface of the ground to appear again from time to time crossing similar valleys and streams upon bridges, the remains of some of which may still be seen, until it reached Soucieu, on the edge of the valley of the Garonne, where are still seen the remains of a splendid bridge, the thirteenth on its course, nearly 1,600 ft. long, and attaining a height of 56 ft. at its highest point above the ground. The object of this bridge was to convey the channel of the aqueduct at a sufficient height into a reservoir on the edge of the valley. The remains of this bridge leave no doubt that it was purposely destroyed by barbarians; some of the arches near the end of it remain, while the rest have been thrown down, some on one side and some on the other, but happily the arches next to the reservoir, at the end of the bridge and on the edge of the valley, remain, and the reservoir itself is still, in part, intact, supported on a huge mass of masonry. Four holes are to be seen in that part of the front of the reservoir which is left, being the holes from which the lead pipes descended into the valley. There must have been nine of these pipes in all. These holes are elliptical in shape, being 12 in. high by 9½ in. wide, and the interior of the reservoir is still seen to be covered with cement. The walls of the reservoir were about 2 ft. 7 in. thick, and were strengthened by ties of iron; it had an arched stone roof in which there was an opening for access. From this the nine lead pipes descended the side of the valley, supported on a construction of masonry, crossed the river by an aqueduct bridge, and ascended into another reservoir on the other side, as seen on the plan, entering the reservoir at its upper part just below the spring of the arches of the roof. From this reservoir the aqueduct

passed to the next on the edge of the large and deep valley of Bonnan, being underground twice and having three bridges on its course, the last of which, the sixteenth on the course of the aqueduct, ends in a reservoir on the edge of the valley. Only one of the openings, by which the siphons, of which there were probably ten, started from the reservoir, is now left. The bridge across the valley below had thirty arches, and was about 880 ft. long by 24 ft. wide. A number of the arches still remain standing, and, as will be seen by the photograph, in some instances the pillars of the arches were constructed of transverse arches themselves. The work consisted of concrete, formed of Roman cement, so hard that it turns the points of pickaxes when ememployed against it, with layers of tiles at regular intervals. The surface of the concrete is covered with small cubical blocks of stone, placed so that their diagonals are horizontal and vertical, and forming what is known as *opus reticulatum*. After crossing the bridge the pipes were carried up the other side of the valley into a reservoir, of which little remains, and then the aqueduct was continued to the next valley, passing over three bridges in its course. This valley, that of St. Irenée, is much smaller than either of the others, but nevertheless it was deep enough to necessitate the construction of inverted siphons, of which there were eight. Leaving the reservoir on the other side of this valley, the aqueduct was carried on a long bridge (the twentieth on its course), which crossed the plateau on the top of Fourvières and opened into a large reservoir, the remains of which are still to be seen on the top of that hill. From this reservoir, which was 77 ft. long and 51 ft. wide, pipes of lead conveyed the water to the Imperial palace and to the other buildings near the top of the hill. Some of these lead pipes were found in a vineyard near the top of Fourvières at the beginning of the eighteenth century, and were described by Colonia in his history of Lyons. They are made of thick sheet lead, rolled round so as to form a tube, with the edges of the sheet turned upwards, and applied to one another in such a way as to leave a small space as shown in the diagram, which was probably filled with some kind of cement. These pipes, of which it is said that twenty or thirty, each from 15 ft. to 20 ft. long, were marked with the initial letters TI. CL. CAES. (Tiberius Claudius Cæsar), and afford positive evidence that the work was carried out under the Emperor Claudius. Lead pipes, constructed in a similar manner, have also been found at Bath, in this country, in connection with the Roman baths. The great difference between this aqueduct and those near Rome arises from the fact that, instead of being carried across a nearly flat country, it was carried across one intersected with deep ravines, and that it was, therefore, necessary to have recourse to the system of inverted siphons. There can be no doubt that the inverted siphons were made of lead, although no remains of them have been found, for we know that the Romans used lead largely, and, as we have seen, pieces of the lead distribution pipes have been found. It is possible, and even likely, that strong cords of hemp were wound around the pipes forming the siphons, as is related by Delorme in describing a similar Roman aqueduct siphon near Constantinople; Delorme also describes, in the aqueduct last mentioned, a pipe for the escape of air from the lowest part of a siphon carried up against a tower, which was higher than the aqueduct, and it is certain there must have been some such contrivance on the siphons of the aqueduct at Lyons. Flacheron supposes that they consisted of small pipes carried from the lowest part of the siphons up along the side of the valley and above the reservoirs, or, in some instances, of taps fixed at the lowest part of the siphons. The Romans have been blamed for not using inverted siphons in the aqueducts at Rome, and it has been said that this is a sufficient proof that they did not understand the simplest principles of hydraulics; but the remains of the aqueducts at Lyons negative this assumption altogether. The Romans were not so foolish as to construct underground siphons, many miles long, for the supply of Rome, but where it was necessary to construct them for the purpose of crossing deep valleys they did so. The same Emperor Claudius who built the aqueduct at Rome, known by his name, built the aqueduct of Mont Pila, at Lyons, and it is quite clear, therefore, that his engineers were prac-

tically well acquainted with the principles of hydraulics. It is thus seen that the ancient Romans spared no pains to obtain a supply of pure water for their cities, and I think it is high time that we followed their example, and went to the trouble and expense of obtaining drinking water from unimpeachable sources, instead of, as is too often the case, taking water which we know perfectly well has been polluted, and then attempting to purify it for domestic purposes.

THE PHYSICAL BASIS OF PHENOMENA.

By H. H. BATES.

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If there is anything entirely disheartening, it is to see the few landmarks of human achievement disappear before the shifting current of opinion, as headlands disappear under the ceaseless buffeting of the ocean. It is no doubt a matter of poignant regret to the cherisher of ardent theological convictions to see the bulwarks of faith slowly undermined by controversy. So, also, to him who has built his convictions on supposed demonstrable and irrefragable fact, to find nothing unassailable, not even the axioms and postulates conceded for ages as first principles, on which the fabric of science was reared, nor the sublime inductions of Galileo and Newton, on which the modern philosophy called natural—the only fruitful philosophy which man has produced—has been founded.

But the course of criticism shows that there are no first principles. Nothing is unquestionable. Even the mathematic joins hands with the metaphysic. I propose briefly to examine the fundamental grounds of mechanical philosophy, in view of the wide divergence of basal hypotheses in recent years, and especially on account of the importance conferred upon certain speculations by their admission into works of standard reference and authority.*

To do this aright it is necessary to go behind the mere sub-science of mechanics to the essence and substance of things, as did the eighteenth-century philosophers succeeding Newton. The observational data which have accumulated since that time by the splendid efforts of the molecular physicists enable us to review and recast, with some promise, the primary dogmas regarding the physical basis of phenomena. It is legitimate to frame hypotheses on subjects which are still unfathomed, but which confessedly do not belong to the domain of the unknowable. The distinguished example of the authors of the vortex atom would alone justify such a conclusion.

No entirely satisfactory hypothesis of the atom has yet been found. I do not design to discuss the vortex atom here at length; for, although it is the most successful form of the Cartesian doctrine of vortical substance, it has not been perfected, and is generally regarded rather as an example of remarkable speculative and mathematical ingenuity, than as a discovery corresponding with any facts of objective physics. It has insuperable difficulties, some of which have been pointed out by Clifford, and others by Clerk-Maxwell. Moreover, unparticled or continuous substance, the necessary postulate in this hypothesis, is something we not only have no experience of, but find full of inconsistencies with experience, when we gain a clear conception of what it implies. Such a conception fulfills Hegel's paradox that being and nonbeing are the same, since it forbids all mobility, all differentiation, as was perceived by the followers of Democritus. It simply affords an inviting basis for analytical discussion, on account of the elimination of the very conditions of objective existence which make the mathematical difficulty.

There are some postulates regarding substance which we may probably be permitted to assume at the outset. We may postulate its objectivity, and also its dis-

* Encylopædia Britannica, 9th ed., articles "Mechanics," "Measurement," etc.

continuity. I have no space to review here the time-worn controversy between continuous and discontinuous substance. The arguments, which are exhaustive from the metaphysical side, are as old at least as Democritus and Anaxagoras. Suffice it to say that modern experiential philosophy has decided the battle experimentally in favor of the discontinuity of matter. The dispute only lingers in the region of the atom, where observation cannot penetrate or has not penetrated. The inability to conceive which attaches to all non-experiential affairs is encountered here, coupled with the too great facility of conceiving what is superficially observed, but will not bear analysis. Thus our first impressions of substance are in favor of its continuity. It is only after much reflection that we get the idea of necessary discontinuity, as bound up with the exhibition of existing phenomena. But the wonderful development of the Cartesian mathematics, in conjunction with the infinitesimal calculus, and its great facility in dealing with geometrical continuities, has tacitly revived the Cartesian idea regarding the nature of matter, as synonymous with space relations, which never reached intelligible development at the hands of its author, and wholly declined and disappeared after the establishment of the Newtonian philosophy, and the discovery of the discrete character of substance.

In point of fact, experience would point to extreme porosity or discreteness as characteristic of substance, rather than to its opposite—perfect continuity. The infinite divisibility of space has nothing in the world to do with the question, though this is a confusion often fallen into. On the contrary, there is an infinite distinction between the infinitesimal discrete units of substance, occupying extension by their interactivity, and the passive infinitesimal resolvability of space continuity. This is the antipodean difference between the Epicurean and the Cartesian conceptions; the former admitting of the operations of force, the free exhibition of motion, the organization of material phenomena, which are phenomena of mobility; the latter constituting a plenum, with only ideal divisions, and phenomenally as necessarily barren a negation as space itself.

Substance is purely experiential. In its essence it is still incomprehensible, because experience has not yet reached down to those recesses. We know nothing of substance except by its manifestations. These manifestations are cognized by us through sense impressions, weighed, compared, adjusted, and analyzed in the mysterious alembic of the mind. First impressions have enormous predominance, and are intensified by heredity of cerebral predisposition and function.

We cognize substance only in bulk by direct perception, and these vast aggregations stand in thought for matter. A drop of water contains incomparably more molecules than the ocean contains drops; a grain of sand more particles than the earth contains grains; and it is this vast mesh of complicated forces that forms the integrated concept of matter to our apprehension. The child, before he can walk, encounters obstacles to movement, reaction to his every muscular effort, of equal measure to his own; and thus his first and profoundest convictions of objective existence are associated with resistance, opposition, repulsion. This impression of matter is so early that it remains with us as its most natural and obvious characteristic.

The idea of weight is also one of the earliest experiences. This idea would not be conceivable to a denizen of the deep sea, for our first ancestor who emerged from the water gained the experience at the cost of great struggle and enterprise. By the natural development of muscle and function the child rears itself very early against the constant pull of our pedestal, triumphs over it with new-found energies, dances on tiptoe, and spurns the ground, but is soon content to draw the battle, to wander around a few weary years on equal terms, at length to call in the aid of a stick or crutch, and, finally, to resign the unequal contest, and sink, vanquished and satisfied, to rest in its bosom. Weight thus seemed a natural characteristic of matter until identified and generalized by Newton as a universal and especially a reciprocal property. This generalization transferred the property, in conception, from the naturally heavy body to a cause outside thereof, namely, the earth itself. Here the human mind reluctd, for, unlike repulsion, attraction is not an observational fact. All forms of tension, stress, con-

straint—by whatever name called—are attended in the child's experience with an intermediary connection. The string is necessary to pull the cart, and the action of the magnet upon the iron particles is viewed with astonishment and awe. The sense of mystery does not proceed so far in his case as to contemplate the equally mysterious power which makes his string differ from a rope of sand. The most profound attention of the human mind has not yet fathomed this mystery.

Inertia or mass is a less obvious property, being in early observation and in common apprehension bound up with weight. It was not recognized in philosophy till Galileo's time, nor is it now by the common perception, except after training. A lady makes no scruple of asking to have a loaded car or train or vessel stopped at a given point on the instant, and reinvested with motion any number of times; and would-be inventors often contrive theoretical machines having numerous heavy reciprocating parts timed to velocities impossible of execution. With beings under other conditions it is wholly different. The sword-fish, *e. g.*, can have no conception of gravity, as he has no perception of it, but his apprehension of inertia is finely cultivated, through the muscular sense, in setting up and modifying the rapid movements in which his existence delights, as well as through his vivid realization of momentum, in the piercing of a whale or a vessel, by which his function is so powerfully exhibited. When once realized by human perception, however, inertia becomes identified with substance as its most primary characteristic.

The old scholastic property of impenetrability. also, is one of the superficial notions of experience, gained in the same way as that of repulsion. It seems to pertain to solids—the typical matter—with approximate accuracy, though calcined plaster of Paris and water, *e. g.*, will occupy a good share of each other's volume, and still form a highly porous solid. But a quart receiver full of hydrogen can have a quart of carbonic acid gas deftly introduced into it as into a void space; and so can a quart of water, at ordinary temperature and pressure, according to Gmelin, without increase of volume, although water is the type of material continuity. As to impenetrability in the molecule, we can predicate nothing. The evolution of heat in chemical combinations indicates penetration of volume, with reorganization of the molecule in less space; and there is no reason, except a scholastic one, why two or more molecules or even atoms, should not occupy the same place, as admitted by the highest authority—James Clerk-Maxwell.

Dimension is also a common notion, derived similarly from superficial and early experience. Solids alone have figure and assignable dimension, though liquids have fixed volume, and gases variable volume, in inverse ratio to constraint; but even solids are of varying and fluctuating dimensions, according to temperature, density, etc. Solidity and liquidity are, it is well known, but mere transitory conditions of material aggregation, for all matter is capable, by sufficient accession of molecular motion, of assuming that hyperbolic or expansive condition which we call gaseous, and in this state dimension and impenetrability are meaningless terms. Concerning dimension, as a necessary attribute of the unit of mass, Clerk-Maxwell says (Encyclopædia Britannica, 9th Ed., Vol. 3, p. 37): "Many persons cannot get rid of the opinion that all matter is extended in length, breadth, and depth. This is a prejudice . . . arising from our experience of bodies consisting of immense multitudes of atoms." That there is no necessary relation between mass and volume as there is, *e. g.*, between mass and weight, is shown to common experience by the notably different masses of a buckshot and a pith-ball of the same dimensions, or of a cannon ball and a child's hydrogen balloon. A pellet of iridium equivalent in mass to the pith-ball might be microscopic, and, by extreme supposition, infinitesimal. We are not forced, however, to deny to the unit of mass finite magnitude, as this would be an experiential fact when ascertained.

The remaining so-called properties of matter are too obviously transitory, accidental, or derivative to require attention. Color, luminosity, opacity, transparency, sapidity, sonority, odor, texture, temperature, diathermancy, plasticity, hardness, brittleness, density, compressibility, conductivity, malleability, fusibility, solubility, and many others, are too clearly but conditions of aggregation, or else mere

subjective states due to the way the complicated interactions of the primary qualities affect our senses. What are the primary qualities?

Here is where the modern method of philosophy flags, by the disappearance one by one of the experimental means of approach, as we eliminate the non-essentials. But though the substance is thus elusory, we cannot yet believe it to be illusory.

Chemical and molecular physics have already gone marvelously beyond the ordinary range of sense-perception, by strictly scientific methods. Not only is the discrete character of matter established, but many data of the differentia and organization of the molecule are discovered. Here is a vast field of science in itself. From the ideal molecule, or simple couple, up through the 70 actual organized molcules of our provisional elements, then the chemical molecules of their combinations in vast numbers, discovered and undiscovered, and, lastly, the enormously complex organic molecule in infinite variety, the domain transcends in area for classification that of biologic science. The simple molecule has not yet been discovered, much less the molecular constituent, the atom, or the *indivisible*. It is evident, however, that the properties of matter which are essential, not differential, must reside in the atom. The philosophers succeeding Newton, treated the atom and the elementary molecule as one, from lack of sufficient chemical knowledge. We are on a higher plane of information, but their method is not necessarily vitiated by such lack of distinction.

We cannot, as before said, attribute *a priori* to the atom dimension or figure, though we postulate it to aid conception. As the atom is an absolute unit, there is incongruity in finally assigning to it such relative attributes, which are but matters of comparison and degree. There are properties, however, which are inseparable from an absolute essence. These are the properties by which the essence is manifested to us. We know them provisionally as forces, in the Newtonian nomenclature. Had gaseous matter neither weight nor mass, we could not know of its existence. But these attributes are so constant in matter that we estimate its quantity in terms of them and have no other exact terms. Weight is the statical measure; mass the dynamical measure. And since weight and mass correspond for all substances, under all transformations, we judge that the correspondence identifies them alike with the essence. They cannot be the mere result of organization. They must belong to the ultimate atom.

At this point it would seem proper to attend to a question of definition. Definitions are essential to clearness, on the one hand, and a source of entanglement on the other, if we fall into the scholastic error of regarding a mere word as the coextensive symbol of an idea. Words are evolved during the imperfection of ideas, and language is still a most imperfect medium of expression. Hence, logic is not a science in the sense that mathematics is. I have used the term force. This is a word of much ambiguity of meaning. We may use it as a convenient mathematical expression for a mere rate of change of momentum, or we may go farther and *define* it as that which changes a body's state of rest or of uniform motion in a straight line; either of which uses restricts it to only a portion of phenomena, and ignores the whole science of statics, dealing with forces in equilibrium and the phenomena of balanced stress. If we give it a more general signification, as that which changes or tends to change, or conserve, the state of motion of particles, or systems of such, either in quantity or direction, we embrace statics as well as kinematics, and get a measurably philosophical definition, if we bear in mind the proviso that we do not thereby postulate force as an entity apart from substance.

And since the compound variable space and time condition, which we call motion (of which rest is but a phase), is the sensible resultant of the interaction of such discrete substance by constant rearrangement where readjustment is free, or the potential resultant where confined, we may admit that the observed tension and persistence, of whatever form, is that which effects the phenomenon (though masked by infinite variety and composition), and always across the discontinuity; not as separate entities, but as modes of manifestation of the interacting and pervasive substance itself and its only manifestations. This we call *force*—the inscrutable agent of phenomena—and this I take to be the true Newtonian conception, as evinced by his maturest conclu-

sions, expressed in query 31 appended to his "Optics."

So far as weight goes, it was generalized by Newton to be a reciprocal force or stress, operative without limit on the law which inheres in radial space relations —the inverse square of the distance. The term operative means effective upon mass, namely, bridging the discontinuity. Gravity is the typical attractive force—*vis centripeta*. The relation is mutual by the law of action and reaction, and amounts to a universal tention among particles, controlling all matter everywhere into orderly movements and relations. This is what we postulate from observation, on the Newtonian plan of naming simply what we see. The notion, however, of action at a distance has encountered a metaphysical difficulty in many minds, from the preconception derived from ordinary experience that all affections or stresses must proceed through an intermediary connection, deemed continuous. Even Newton made concession to this prejudice in his oft-quoted letter to Bentley. That there is really no such continuity in any mode of connection known is demonstrable, and the notion itself that the fancied continuity of some rare effluvium could in any way aid the mechanics of the problem is chimerical. Clerk-Maxwell, moreover, has shown (Encyclopædia Britannica, Vol. 3, p. 63) that action at a distance is as necessarily implied in repulsion as in attraction, so that theories of repulsion do not aid conception. Ability or inability to conceive, furthermore, is not held even by the metaphysicians to be a criterion of objective truth. Such truths exist independent of the conceiving mind. The conceiving organ was evolved by experience, and conception develops with attention. The first law of motion was wholly inconceivable to the contemporaries of Galileo, and we find such instances even now. Thus, while plain truths are inconceivable until established, some utter absurdities have been deemed conceivable, as, for instance, vacuity of two dimensions. State of mind, then, is no measure of external truth.*

The second force or manifestation of the atom, inertia—or mass—unlike gravity, is not unlimited in range of action. As to this property matter is discrete. Mass has both a *locus* and a limit (being apparently dependent for dimension on multiplicity), and amounts to that incomprehensible property by which conservation of motion is maintained. Under gravity, quantity of motion varies according to relations of contiguity, but under inertia motion is conserved in direction and quantity, is modified in direction and quantity by interaction of mass with gravity, and is redistributed by interaction with repulsive force upon an indefinitely near approach of particles, upon conservative principles. Its discreteness gives matter its numerical and finite character, and admits of that interplay which constitutes phenomena. Its reality and primary character, when once apprehended, have proved more acceptable to the imagination than has the conception of central force, and under appulsion hypotheses (with the aid of that other readily accepted property, repulsion, and certain highly artificial hypothetical media), it has been made to do duty in providing so-called explanations of gravity, under its form of *vis viva*.

It has always seemed to me that the mode of approach adopted by Boscovich was the most philosophical and rigorous of any. He viewed matter for the purposes of mathematical treatment and for investigation of its essentials, as divested of accidental and fugitive properties; and as the analytical calculus had not then become so developed as to wholly fasci-

* In this connection, to illustrate how entirely a matter of opinion or prejudice or culture is this notion of conceivability, I quote from a letter written by Faraday to Dr. Playfair, in response to some inquiries of the latter about his atomic opinions:

. . . "I believe in matter and its atoms as freely as most people—at least, I think so. As to the little solid particles which are by some supposed to exist independent of the forces of matter, and which in different substances are imagined to have different amounts of these forces associated with or conferred upon them, . . . as I cannot form any idea of them apart from the forces, so I neither admit nor deny them. They do not afford me the least help in my endeavor to form an idea of a particle of matter. On the contrary, they greatly embarrass me; for, after taking an account of all the properties of matter, and allowing in my consideration for them, then these nuclei remain on the mind, and I cannot tell what to do with them. The notion of a solid nucleus without properties is a natural figure or stepping-stone to the mind at its first entrance on the consideration of natural phenomena; but when it has become instructed, the like notion of a solid nucleus apart from the repulsion, which gives our only notion of solidity, or the gravity, which gives our notion of weight, is to me too difficult for comprehension; so the notion becomes to me hypothetical, and, what is more, a very clumsy hypothesis." (Playfair's works, vol. 4, p. 84.)

Here we see a difficulty opposite to that usually encountered, for, while many people express an infirmity of conception of the forces apart from the imaginary vehicle, Faraday finds the vehicle of no use as a carrier of the properties, but a positive impediment.

nate the attention of geometers with abstract and ideal relations, he proceeded from prime physical data. He thus identified matter by those apparently general and characteristic properties recognized by Newton as the basis of mechanical philosophy in conjunction with the laws of motion. These properties are, as before said, gravity, inertia, and repulsion; or, as characterized by function, attraction, conservation, distribution. In this view, matter consists of certain *loci* of central forces, mutually attractive by the first property according to a variable law in the duplicate inverse ratio of distance without limit, but restricted in manifestation as to the second property to the infinitesimal *locus*, thereby excluding unitary dimension. Contemplating matter under this aspect alone, a dilemma arose. For gravity waxing by the law of inverse squares of the distance up to the focus or origin, involves the consideration of infinite force and apparently of infinite velocity in the limit, in the supposable case of rectilinear approach, at which point the equations become unexplainable. While Euler and La Place differ in their interpretations of the result, Boscovich sought to solve the apparent absurdity and inconceivability by the invention of his ingenious and complex system of alternate spheres of attraction and repulsion, or change of sign, on a very near approach, with infinite repulsion at the focus, which so loaded down and vitiated his hypothesis as to cause its rejection. This result was similar to that of Le Sage's speculations and those of the Ptolemaic astronomers, each thus working out the falsity of his respective scheme by superadded complications to readjust the theory to the progress of criticizm or of observed fact.

By attributing finite magnitude to the atomic mass, however, Boscovich's difficulty disappears, as I had the honor of pointing out before this Society some ten years ago. This may be deemed a violent hypothesis in regard to a positive discrete simple absolute, as the atom is presumed to be, but parallel difficulties inhere in any other finite supposition, as, *e. g.*, a sphere of repulsion. Under my provisional assumption, the way out follows from an elementary proposition of Newton's, and it does not demand the gratuitous change of law or of continuity involved in the resort of Boscovich. The movement of a gravitating particle under stress of a center of gravitative force would be in all respects as the great 18th-century mathematicians have demonstrated, until the margin of the particle reached the attracting center, where, if we suppose the attractive virtue to pervade the particle equally throughout a certain finite volume of mass, however minute, as gravity does the mass of a sphere, the maximum of attractive force would be attained; for, as Newton has shown, homogeneous spheres are controlled under gravity by a law of force varying directly as the mass and inversely as the squares of a distance between their center of mass and the attracting center, at all points *beyond* the surface, and directly as the distance between the said centers *within* the surface; so that after passing the surface, the attractive center must proceed onwards to the gravitating center of mass (relatively), not by a force increasing to infinity, but by a force decreasing to zero, after passing the maximum, since it is balanced at the center by opposing stresses.

A similar law of attraction prevails between two gravitative particles when both are similarly endowed with finite spherical volume and mass, excluding the idea of impenetrability (which is not a necessary attribute of mass), the Newtonian law being the product of the masses divided by the product of the distances $\left(\frac{Mm}{dd}\right)$* for outside positions.

For positions of encroachment the law

* I write the formula this way because it is possible that we have been in error all along in regarding the denominator as a radial space relation, as implied when we write it $\frac{Mm}{d^2}$. In discussing the deflection of the particle under gravity, Newton, for matematical simplicity, treated it as governed by a fixed attracting central force, and in testing various relations found that the radial space relation gave the true path of the planetary bodies under the immense preponderating influence of the sun's mass. The fixed center of attraction is, however, a mathematical, not a physical condition, and can only be realized by making $M=\infty$, when we get a form of expression which does not give a law of force. I think it possible that the relation is a mere reciprocal distance relation, since the stress is mutual for the masses and each is equally distant from the other. The inverse form of the relation, moreover, may arise from our subjective way of viewing distance, as measured outwardly from ourselves, since we have to go from here to yonder. It is possible to look upon the relation as really one of contiguity or nearness, and by placing $\frac{1}{d}=c$ we get the cosmical law of gravitation as $Mcmc$. This, however, would not be a useful formula, since we are not accustomed to expressions which attain maximum value with minimum magnitude.

is more complicated, and forms an interesting field for mathematical discussion. Where three or more atoms are superimposed the problem becomes too complex for discussion. It is noted, however, that such compound atom, if quiescent from extreme abstraction of heat, would be in a condition of elastic equilibrium, ready to respond like a bell to the slightest disturbances. In all these cases of interpenetration the law of stress would be finite and diminishing, and if the line of encounter should chance to be a right line through their centers (a condition infinitely rare in actual occurrence), they would continue on or repeat according to energy of approach; while upon any other lines of approach orbital relations would supervene, in modified curves of the second order, either hyperbolic, parabolic, or elliptic, according to velocity, and with or without partial penetration, according to nearness of approach.

Boscovich, however, did not adopt this solution, although within his reach. The problem of the action of a gravitative particle as controlled by an attractive center has several aspects of statement, which may be confined to four, for practical investigation. In the first, where the particle is assumed to be without mass, no discussion is possible, for the two suppositious points instantly assume the same locality, and end the relation. In the second, where the particle is endowed with inertia but not magnitude (and the attractive *locus* fixed by postulate), the element of motion enters, but infinite terms appear in the equations in the limit, forbiding interpretation. Thirdly, when we attribute finite magnitude to the gravitative particle for gravitative pervasion, as in actual spherical masses, no infinite terms appear, and we get an intelligible mathematical discussion, with planetary results for exterior positions, and pendulum results for interior positions, as I have heretofore demonstrated; and lastly, when both the gravitating *loci* are invested with similar attributes of volume and of mass (excluding extraneous notions of ordinary collision and repulsion from the problem), the results are similar to those of the third hypothesis. I do not introduce any of the mathematical discussions here, as the dynamics of the particle have been fully treated by mathematicians, though I am not aware that any of them have pursued it to physical conclusions.

It is not likely, however, that there is any matter so simple as this modified Boscovichian atom; that is, which can be identified. All the matter we know of is already compounded and highly organized. The ideal simple molecule would consist of a single pair of such atoms, bound to each other in orbital relations of more or less eccentricity, including the extreme rectilineal form of simple pedulum-like oscillation through one another's centers; and it is a most significant fact that spectroscopic observation of all incandescent matter shows atomic matter to be in this state of transverse or orbital oscillation with inconceivable but synchronous rapidity without regard to range, according to the pendulum law of stress varying directly as the range of oscillation, discovered by Galileo. Any theory of the simple molecule must take cognizance of this observed fact. Another cognate fact is that the law of elastic cohesion manifest in all elastic tensile action—"*ut tensio sic vis*"—is a parallel law of stress, as illustrated in the spring balance weighing scale, the spring dynamometer, the isochronous spring governor, etc., and is a function of molecular and ultimately of atomic force and distance.

If the atom is really thus characterized, the repulsion or resistant property experienced in matter becomes worthy of investigation, since it drops out as the primitive affection or disaffection postulated by Boscovich. I have shown that it is not necessary to oscillatory motion. We must admit that the notion of rebound or recoil, in the ordinary sense, between simple atoms possesses difficulties. No less does the idea of plasticity or destruction of momenta. Consider what is involved in the hypothesis of two absolutely hard, rigid, unparticled, homogeneous spherical bodies of any magnitude at all, if possessed of mass, meeting on a rectilineal central line of motion. We know what would happen in case of ordinary spherical elastic masses or aggregations of molecules. Such merely undergo, first, apparent contact, then compression, deformation, strain, accumulation of stress, retardation of velocity, momentary arrest, acceleration on new lines of departure, relief of strain, recovery of form, redistribution of momenta, and final resumption

of uniform velocities, with relative motion inverted and aggregate energy of motion unimpaired, unless permanent distortion and heat have absorbed a portion. All this complex action is involved in the term elasticity. None of this could take place with simple undifferentiated particles, unless we invent for them a mystic atmosphere or cushion of repulsive capacity surrounding the *locus*, as Boscovich was forced to do by logical conclusions. Without this, contact would be absolute and instantaneous at first impact. As hardness involves impenetrability, absolute destruction of motion on the instant must ensue; that is, motion and no motion at consecutive instants of time; a discontinuity unknown to experience, and known to be inconsistent with the nature of motion and of time. This argument from breach of continuity is due to Leibnitz. Conversion into heat motion is excluded, heat being a mode of motion of the entire atom. Moreover, the destroyed motion has to be recreated instantaneously in new directions, for destruction of energy cannot be postulated. This geometrically angular motion is also unknown to experience, for all deflected bodies pass by continuity from motion in one direction into a new direction, and so far as we can see, must do so. These discontinuities in translatory relations are therefore put aside, not because they are inconceivable, but as illogical and non-experiential. Simple repulsions by contact without occult intervention is a false suggestion, and we find that we get the pseudo-conception from our false observation of what occurs in the collision of sensible masses, somewhat as we make a false observation and generalization about material continuity, or about tension, from a superficial perception of matter; thus creating concepts from supposed experience which can have no true objective counterparts. I shall recur later to a possible derivative basis for repulsion.

It is remarkable that to Newton we owe the final establishment of the majority of those fundamental and universal truths which by simplicity and generality seem to touch the absolute; that is, more than to any and all other philosophers combined. Thus, of the six ultimate generalizations, four were formulated and placed on an impregnable basis by Newton: the three laws of motion and the law of gravitation. All of these were inconceivable when first promulgated, were hotly controverted on the metaphysical plan, were finally established experientially, and are now generally accepted as axiomatic by the modern mind, except for sporadic reversions which appear now and then to deny their actuality and reassert their inconceivability. The remaining two universal inductions are the collective group of axioms formulating the relations of extension—the only enduring remnant of the Greek philosophy—and the law of the conservation and unity of energy, unperceived in Newton's time in its generality, though taught as a dogma by the Cartesians. These also are still held to be inconceivable by certain disciples of metaphysical methods and axiomatic by others. Such mental attitudes should lead us to believe that simplicity has been arrived at in all these cases and the boundaries of explainable knowledge reached, where inconceivability necessarily begins.

It has been said that paradox is born either of confusion of thought, or of knowledge, or confusion of statement arising out of the imperfection or subtlety of the verbal vehicle of thought. Thus, as Clerk-Maxwell points out, the celebrated arguments of Zeno of Elea, establishing the inconceivability of motion, represented in the paradox of Achilles and the tortoise, were unanswerable and unanswered until Aristotle showed, some half-century later, that duration is continuous and incommensurable by numerical methods in the same sense that extension is. The old logical dilemma of the irresistible force encountering the immovable body was insoluble to the Greek mind, both from lack of physical knowledge and lack of verbal clearness of statement. The acute sophist knew not the nature of force, the constitution of bodies, the conservation, transformation, and dissipation of energy, and consequently knew not the refuge and escape from the dilemma contained in the perception of the conversion of molar energy into heat energy, expansion, and dissipation. The resources of verbal subtlety and of inner consciousness failed, as they always do. Something of the same difficulty remains in modern problems, where observation and strict verification are, from the nature of the problem, inappli-

cable, or where the confusion arises from the still-existing imperfection of language, or, again, where generalizations, both clearly made out and clearly formulated, have not passed into the instinctive popular apprehension. The modern dilemma of the inconceivability of infinite or finite space is, I take it, due to the metaphysical form of the statement. For when we reflect that the ideas of immensity and of infinitesimal resolvability are but abstract generalizations of the merely relative continuities, extension, distance, and dimension, which are in their turn but abstractions of the sense-perceptions, form, translation, and volume, the statement becomes intelligible and entirely conceivable, and I think, though, with deference, saves geometry; that is, the universality of that system of inductive postulates regarding the relations of extension and inferences therefrom, known as geometry to the Greek philosophy, but now named Euclidean by certain analysts whose so-called geometry is symbolic. Geometry is therefore able to deal with all aspects of extension, without regard to limit, in spite of some infirmity in the Greek methed, for scale cannot affect the generality of extension relations, and abstract unconditioned space is not an entity, but a mere negation, concerning which relative propositions are unintelligible. A false philosophy regarding space is at the root of all modern heresies concerning geometry and mensuration, founded in misapprehension of the Euclidean inductions or generalizations.*

The first law of motion is but the formulated recognition of inertia, which is only manifest in conjunction with motion, actively or passively. It was known to Galileo, and laid down by Descartes as a law in his "Principia." It is a cosmical truth, bound up with the absolute nature of mass and the true relations of extension, which correlates the whole fabric of dynamical knowledge with rectilinear geometry, curvilinear motion being demonstrably not a simple state of conservation under inertia, but a resultant of multiple forces. The simple action of mass under the first law of motion, if undisturbed, furnishes the absolute unreturning rectilineal path which overthrows all speculation about possible ideal spaces. I here recall a book written by a learned American of Philadelphia—learned, that is, according to the medieval standard of the colleges—and published only during the past year, entitled "An Examination of the Philosophy of the Unknowable, as expounded by Herbert Spencer," wherein he naively lays down the first law of motion as unintelligible except by appulsion. Motion, he says, in the absence of propulsion is inconceivable. I have no space here to reproduce the explanation evolved out of consciousness by this reasoner to account for the action of a ball struck by a bat after leaving the bat. It resembles in ingenuity and gratuity some of the inventions devised to explain gravity. The notable thing about it is that here, at this date, is a mind of good caliber, informed in the higher schools of learning, which is still of the mental period of Aristotle; a mind which has evidently never apprehended inertia, nor heard of the great contributions to knowledge made by Galileo and Newton, by which philosophy was entirely revolutionized.

The second law of motion, regarding the independence and coexistence of motions, on which we occasionally see comments in the metaphysical vein controverting its possibility, has long been established experientially. Its early experimental proof is attributed to Galileo. Yet I recall a pamphlet written and published only during the last year by a learned German at Leipzig, the theme of which was that "the sun changes its position in space, therefore it cannot be regarded as being in a condition of rest." This, he concludes, overthrows the entire fabric of Copernicus, because the planetary orbits in such case cannot be closed.

The third law of motion is but formulated reciprocal stress, in its modes of

* There are two opposite though similar forms of error in the assumptions regarding space. The first is that space is a specific or perhaps generic entity or objectivity *per se*, possessed of conditions and attributes, like substance, such as dimension (in several), differentia in locality, figure, as curvature, etc. (hence necessarily finite), and only uncognizable by us simply for lack of perceptive faculties to correspond. This is the fundamental error, as it seems to me, of Riemann and Lobatschewsky. The second is that of the older Cartesians, who viewed space as but the mere attribute or synonym of substance, and inconceivable apart from it, so that bodies separated by void space would be absolutely in contact without regard to distance. Both of these speculations are purely metaphysical, and non-experiential, the latter resulting from the old scholastic method of syllogistic deduction from primary postulates of verbal definition, and the former from similar inferences from the forms of the analytical logic of symbols, the use of which is still in the scholastic stage. Like Zeno's paradox, these merely intellectual difficulties should be removable by intellectual processes.

compulsion and repulsion, through which mass acts on mass to redistribute motion by what appears to be necessary law. The stress is necessarily reciprocal, since there is no *point d' appui*, or fixed fulcrum in the universe.

We have thus been brought to the boundary of the absolute, where all is inconceivable until found out, and where the simple data are unexplainable. All examination seems to continue to point to mass and weight as the ineffable simple insignia of substance standing on this limit. We must accept something as elementary fact; what shall we find more elementary? Repulsion is still debatable; for, if we make an issue between repulsion and compulsion as contradictory primary attributes of the same essence, or untenable in conjunction for artificiality, by far the greater difficulties attach to the former, some of which I have already alluded to. The profound mind of Boscovich was forced to accept repulsion as a primal quality, but in deference to the physical hypotheses of his time, he overloaded it with complication. This has been weighed in the balance of the philosophical judgment and found wanting. I have intimated that there are possible grounds for surmising that it may not be a simple property of the atom, but a mere mode of distribution of energy dependent on composition of motion of atomic mass after change of sign, *i. e.*, a mode of *vis impressa* after exhaustion of the space relation; for, mathematically, the hyperbolic lines of approach and recession of two atoms under high proper motion characteristic of the atom, and on lines not directly central, would be similar, at sensible distances, in their asymtotes (which would be the practical paths), whether the deflection were due to attractive or repulsive stress, though acceleration and retardation at the passage of the infinitesimal focus would be inverted.

It therefore seems to me immaterial to result which of the two modes of passing the infinitesimal focus is the true one. In either case the distance at passage is infinitesimal, and the force may be as near infinity as the facts require it to be assigned. The normal or rectilineal encounter is here excluded from supposition. In that case, under repulsive stress, as postulated by Boscovich, the recoil would be rectilineal and opposite, without breach of continuity. Under attractive stress, with finite volume of the atomic mass, penetration would ensue as before shown; but without dimension or repulsion we have an insoluble condition, although the occurrence would be infinitely rare. Only one pair of elements is here considered. In all real encounters, whether of masses or molecules, the effect is a vast resultant, but should not be different in kind from that of the elements; that is hyperbolic or expansive between alien systems under motion. As the number of elements ordinarily engaged could not be represented by any numerical places of arabic notation for which we have names, we see the hopelessness of stating the problem mathematically. I therefore do not presume to offer this as an explanation of repulsion, and I confess that to me repulsion is in its mechanism incomprehensible. We know the result experimentally, and that is resistance to penetration, and reaction at insensible distances on an undefined boundary which begins prior to contact and increases in a high exponential ratio as approximation progresses. The contact boundary of any solid—even the smoothest and hardest—resembles the astronomical limb of Jupiter is geometrical indefiniteness. The contact transmitter in the telephone, the whole range of whose phenomena occurs under pressure and so-called contact of varying degrees, illustrates how relative a thing is contact. Under high velocities the distinction between solids, liquids, and even aeriform bodies entirely disappears in repect to repulsive reaction, though this is the most sensible distinction between them under low velocities.

We may, therefore, adopt the conclusion that if any of the apparently simple properties of the atom are to be thrown out as derivative and secondary, presumption points to repulsion as the complex one. We could possibly account for phenomena in a universe bound together by purely tensile stress, but most of the sensible phenomena of solids—cohesion, affinity, tenacity, etc., including nearly all of statics—remain hopelessly unattackable problems under a hypothesis of pure repulsion, like that of Le Sage, or Preston. It is to be noted that the kinetists who freely postulate repulsion and appulsion, without analysis, as a primordial fact, but relect against compulsion or tension, are

forced to the invention of the most complicated and gratuitous mechanism and media to explain the phenomena of gravity, and then without attainment of result. Le Sage's atom is too complicated, even without his suppositious or extra mundane operative machinery; and the vortex atom is but a mere analytical expression for an unproducible condition in a figmentary mathematical plenum.

The thesis that conservation is the characteristic by which we identify objective existence will not bear the test of examination. It is only in the most recent times that such a quality has been known or imagined, and its establishment, both as to matter and energy, is justly viewed as the triumph of modern philosophy. The evocation of matter from nothing and its relegation to nothing, even by the finite will of a wizard, was ever a common and universal notion, which did not at all impair the belief in its present reality and substantiality. We have only to go to Apuleius for this, and it is doubtful if even now the notion of the indestructibility of matter is anything but a scientific conviction, for do we not see numbers of our contemporary fellow-citizens meeting together frequently in our midst to witness feats of materialization out of nonentity by powers akin to those of the sorcerer, without an idea of incongruity? Nor has the essentially modern doctrine of the conservation of energy anything to do with the belief in its reality. Few people apprehend it even now. No philosopher understood it a hundred years ago. Its verity rests on a sufficiently general inductive basis, from the refined and exhaustive experiments of Joule, and the theoretical conclusions of Mayer and Clausius, and it is accepted in the same sense that the law of gravitation is accepted. But the duality of matter and energy to the exclusion of force is a verbal shift, the assumption of which removes no difficulty. Matter, the object, remains unexplained; and energy, the phenomenon, becomes segregated and unintelligible. Energy, in fact, is but mass in phenomenal manifestation, being a product of triple factors, two of which—translation and speed—are not things, but variable and evanescent conditions, and, taken together, constitute motion. Mass is the absolute or persistent factor, but the evanescent character of the variable component—motion—would render the entire phenomenon—energy apparitional, were it not for the distance relation involved in motion, which, under the same inscrutable agency which modifies and saps the motion, renders it potential upon change of sign. This agency, the dynamical source of the manifestation, being central to mass and likewise pesistent and constant, renders the positive and negative potentialities of movement constantly equal, and the actual and potential energies consequently complimentary, from which energy gets its character of conservation.

Energy cannot therefore be that other reality of existence (besides matter), since force is clearly the one reality at the bottom of the manifestation of both, to whose persistence and resistance to change, except through tranformation, the conservation of both is due. This one reality is, in its triple aspect of causation, (1) attraction—the source and modifier of motion; (2) inertia—the conserver of motion; and (3) repulsion—the distributor of motion; or, more correctly, in its aspect of quality: (1) *vis centripeta*—the power of mutual control across distance; (2) *vis insita*—the power of persistence in state of motion impressed; and (3) the distributive power of imparting and acquiring motion by transfer, at minimum distance, which may be called *vis partitiva*, the result of which is Newton's *vis impressa*. Matter thus comes into the world of phenomena by the simple presence of other matter, permitting the exhibition of these comparisons and interactions, involving the conditions of contiguity, distance, position, translation, direction, succession or sequence, and time-rate for the continuous increments, decrements, successions, and uniformities, all bound up in the compound variable continuity—motion. With motion and distance comes the dependent phenomenon—energy—active and potential, which should be a constant, the numerical units of mass being constant throughout immensity, provided the sum of the motions, potential and actual, be constant. This the dynamical theory deduces from the fact of central force (for without force potential motion is ridiculous), and the thesis of the conservation of energy is a dynamical truth or nothing. It is therefore all the more

extraordinary that certain kinetists, who reluct against central force, should have selected, out of all the manifestations of the universe, the variable and conditional product—energy—to be the one reality or objectivity, aside from the undefined hypostasis—matter—as a primordial simple fact at the basis of phenomena. It has been mathematically demonstrated by Mr. Walter R. Browne that the conservation of energy is true if the material system is a system of central forces, and is not true if the system is anything but a system of central forces. In fact, the ordinary theoretical proof of the principle of the conservation of energy assumes the forces acting to be central forces, *i.e.*, reciprocal stresses between units of mass, as recognized by Clausius in his "Mechanical Theory of Heat." Moreover, the entire body of kinetists, who have aimed to supersede gravity or central force, have freely assumed an extramundane supply of motion and energy without regard to conservation, and it is notable that every hypothesis for this purpose yet broached involves the constant expenditure of work without recovery, and postulates the accession of energy in infinite influx from some occult source, of which only a small portion relatively is available or manifest in observable phenomena, thus violating all three of the canons of philosphical ascription—true cause, sufficient cause, and least cause. Such is the power of conception of the unknown in endeavor to explain the inconceivable known.

If the dynamic hypothesis of perpetual transformation of energy could be established as a universal induction, with as much generality *e. g.*, as the statement of the law of gravitation, it would establish and confirm that law, by Mr. Browne's demonstration, as something more than a law, to wit, the necessary constitution of matter as a system of central forces and nothing more, substantially as conceived by Newton and elaborated by Boscovich. At present it is but a dynamic induction, but the theory of gravity is no more. Our appliances are material, and we can deal with molar forces, but only indirectly and inferentially with those which are atomic Conservation is indubitably true of energy in the mechanical and molar sense, under the laws of dynamics and the persistence of force. It is, also, experimentally true, so far as we can trace it, of those less understood forms of energy which are molecular or atomic, the establishment of which was the great glory of Benjamin Thompson, Clausius, and Joule as to heat, and of a multitude of observers as to electrical energy. We infer it as a general truth of these energies (formerly known as imponderables, since they are not manifestations of matter in the concrete), from the fact of their convertibility with other modes of energy which are undoubtedly dynamical, and also from the intimate connection of electrical energy with one of the specific exhibitions of central atomic force—magnetism. Such clews create a warrantable presumption that the phenomena in question will all ultimately be classified among the modes of atomic mass and motion, inductively as well as hypothetically. Possibly in the investigation of these evanescent modes of energy the missing simple particle may come to light. Provisionally, we are entitled to rank them among the mechanical modes of energy, as products of the same material forces, assuming, until the contrary is proved, that some form of matter is concerned in manifestations so correlated by conservation with undoubted material activities.

In including the imponderables within the general dynamical law of conservation, we have to take account of the phenomenon of dissipation, first pointed out by Sir William Thomson. It is true that heat (as well as electrical energy) is strictly correlated with and interconvertible with energy of mass motion, as before stated, but in its final form energy seems to take leave of matter altogether, so far as our perceptions can follow it, and disappear as a material phenomenon (though liable to reappear wherever matter is encountered whose particles are deficient in a like species of atomic motion with that which disappeared; which fact indicates that atomic mass is still a factor, with its inherent property of persistence and transference). The earth and all upon it is radiating heat energy away into space at the constant rate of 500° F. of absolute temperature, more or less; the sun and the visible stars at the rate of many millions of degrees. Much energy also passes off in the luminous form. Of electrical and actinic energies

we know less, and of some we doubtless know nothing. This amounts to a constant drain of the dynamical supply of energy. These final forms, the radiant energies, have a remarkable specific high cosmical velocity of their own, which is a function of something not material, or at least not molar. It is supposable that, in addition to the dynamical source of motion from central forces, and the contraction of systems in dimension which supplies dissipation, there may be an inherent and primordial store of atomic motion. The high proper motion of some of the stars, beyond what can be accounted for on dynamical principles, and the inexhaustible and enormous supply of radiant energy from the visible stars, have afforded grounds for such a surmise, but these speculations do not belong to the domain of mechanics.

And here we must bear in mind that the dynamical theory, in placing these assumed agencies and modes of interaction in causal relation to phenomenal motion, by no means predicates or can predicate anything concerning absolute motion or its cause. The lack of this distinction may have proved a stumbling block to some in comprehending the idea of force. Were it not for the observed dissipation of energy no system could become contracted in dimensions a particle by the interactions of material forces, nor is there now any known way by which the material system can be expanded in dimensions except by the accession of motion from extra-mundane sources, which there is no scientific mode of ascertaining. The sum of motions under the action of forces remains the same, and any change would imply creation or annihilation, which is not ascribable to a material agency. Primordial dimension remains as inscrutable a fact as ever, and primordial motion an unsolved problem.

In conclusion, I know nothing of force except as a manifestation of matter, and nothing of matter except through its manifestations. It is substance that interacts with substance, so far as we know, always reciprocally, and force is but the convenient translation of the terminology invented by Newton to designate these several species or modes of action, in the word *vis*, with its appropriate adjective. He was arraigned by the Cartesians (and virtually is by their modern representatives) as the reintroducer of occult qualities into philosophy, but his statement was "*hypotheses non fingo*," and to a similar charge brought against him by Leibnitz he pertinently replied that it was a misuse of words to call those things occult qualities whose *causes* are occult though the qualities themselves be manifest.

I have adopted gravity as the type of central inherent force—*vis centripeta*—but I would not thereby be understood as excluding from the category of material forces any and all other modes of tensile or constraining force which may be hereafter made out as specific, by the elucidation of such phenomena as affinity, cohesion, tenacity, elasticity, ductility, viscosity, capillarity, polarity, magnetism, etc., now so little understood, any more than I would exclude any form or mode of energy which may be observed, from the category of material phenomena. The Newtonian doctrine of force would not be impaired by such discovery, and its strength lies in the fact that it as readily includes static phenomena—that despair of the kinetist, who has no imaginable hypothesis by which to range them under a form of motion—as it does kinematical phenomena. Statical force (Newton's *vis mortua*) cannot be ignored in a theory of force. The straw that breaks the camel's back—the very lightning that crashes through the sky—are familiar examples of its power made manifest. Its reality may be exemplified by suspending two heavy balls of equal weight at equal heights—one by an elastic cord, and the other by a tense string. The difference of effort required to displace the two vertically upwards, which can be measured, makes sensible the difference between the two forms of balanced statical forces. In the one case the antagonizing force is suddenly withdrawn, and in the other gradually. Wherever strain exists—and it is everywhere—there force is as certainly present as when it becomes manifested in a stress relieved by motion and measurable in terms of energy.

Let us, then, give up the standard of *a priori* conceivability, in view of its many historical failures, and adopt as possible that which is provisionally ascertained. The "ego" and the "cogito"—Cartesian starting points—have proved barren and

irrelevant in philosophy. True philosophy is concerned with objectivity. The data of consciousness, mainly acquired in infancy or in the womb, are blind guides. Many an ego, whose brain was his cosmos, has run through his brief subjectivity, but the order of nature endures. The same facts are continually observed, verified, recorded, and rectified, but the observers change. Their intelligent observations add to the sum of knowledge. This is all the proof we need of objectivity, and all we will get. The insoluble difficulties of philosophy have disappeared one by one since the happy thought of eliminating them by observation entered. The immortals are those who have successfully applied this method. It is only where observation fails that insolubility lingers. Beyond the sphere of the knowable it will continue, in spite of introspection. How masterful is fact in the presence of the most intricate mental subtleties. The ball leaves the bat, in spite of the inconceivability. Galileo's plummet dropped from the moving mast strikes the deck and not the water, in spite of the inconceivability. The Earth returns in its orbit, to the second, in spite of the sun's rapid fall through space, and of the inconceivability. Two opposed horses can pull no more than one, in spite of the inconceivability. The guinea and the feather dropped in the exhausted receiver strike the plate together, in spite of the inconceivability. The isochronous pendulum swings through the widest arc in the same time as through the smallest, in spite of the inconceivability. The minute hand overtakes the hour hand, in spite of the inconceivability. The magnet draws the iron with undiminished force through all possible interpositions, in spite of the inconceivability. Could an exception be found, the perpetual-motion "crank" would work a greater inconceivability, by the instant contrivance of a power-generating machine.

We need not aspire, therefore, to remove any of the inconceivabilities of the external world. We must accept them as natural to the finite comprehension, as necessary to faculties which act by comparison, and above all as evidences of objectivity. On the other hand we should avoid that opposite error of the introspective school, of deeming that probable, or in any way connected with fact, which merely seems conceivable. I have shown that while the simplest truths have generally proved inconceivable until found out and established by genius, the greatest absurdities have had ready currency without a doubt of their conceivability. This all mythology shows. Such rubbish as "a thing cannot act where it is not," and "a body cannot move where it is not," or "a cause cannot precede in its effect"—mere metaphysical assertions or subtleties in face of every-day fact—were stumbling blocks for ages. Such assumptions formed the basis of deduction in lieu of observation, and blocked the possibility of advance. And even yet, rigid deduction from the most hare-brained premise, if the chain of deduction is sufficiently intricate, seems to possess fascinations over a verifiable induction, with many minds.

And now, if any ask, "*cui bono*," to the scientist these philosophical inquiries and intricacies, when he has the vast field of unexplored data still before him to occupy him, I answer, the queries of philosophy are not only the main-spring and final cause of science (her first fruitful daughter and handmaid), but they conciously, or unconciously, dominate the methods and results of science herself. Each investigator, even though in the domain of the most abstract of the sciences, postulates more philosophy than he is aware of; and with so much the more danger to final accomplishment if he assumes his philosophical basis without examination. It is the errors of giant minds that are dangerous, by their ponderosity. The infallibility of the master, Aristotle, seemed to make investigation useless, until the rise of parallel giants, like Galileo, and Copernicus, stimulated a new conflict of opinion. And Descartes, though harmless from all his productions within the metaphysical domain, is dangerous by his very eminence and originality in science, which gives vogue and currency to his monumental errors. Although acquainted with the true law of motion, his scheme of matter evolved from conciousness would forbid all exhibition thereof. A grand geometer, he erected a scaffold for scaling immensity, and with unparalleled penetration perceived how a purely ideal logic, if general, would represent truth in a wholly dis-

similar realm of deduction, if equally general. Strange to say, this grand and useful discovery has become the engine, in nihilistic hands, for overthrowing all the positive knowledge we possess—the achievements of two thousand years of human effort. Not only geometry—all that has survived to us of philosophical value from the antique world—but the basis of positive dynamics, as handed down from Galileo and Newton, and Boscovich and Dalton, are apparently undermined, for all that gives them intellectual value—their certainty—unless an effort be made in the neglected field of philosophy. With strange inconsistency these advocates *par excellence* of the experiential origin of knowledge are found in the same breath promulgating as possible truth matters not only non-experiential, but not representable in ideas derived from or verifiable by experience, and avowedly originating, not from inductive generalizations—the only source of knowledge—but in purely deductive processes in the old scholastic way, from logical premises of bald assumption. In a similar way, in the hands of the Greek sophist, language, a good servant, became a vicious master, and made a chaos of all ethical achievement. A remnant of knowledge, fortunately expressed, not in verbal, but diagrammatic logic—geometry—was left, but only to fall now by the hands of similar iconoclasts, armed with more potent destructiveness, in its full flower and fruit of twenty centuries of unmolested growth.

It is time, therefore, to get back to Baconian ground, and while using for its legitimate purposes the magnificent modern machinery of analytical investigation in the field of abstract continuity—extension, motion, duration—not attempt to conjure with it as a source of objective revelation, which no mere machinery can be. A scaffold of n dimensions is as useless to the geometer as to the architect. To assume matter as continuous, simply because of the possession of a potent engine for the investigation of continuities, is to repeat the practice of certain quack specialists, who are prone to diagnose nearly every form of disease as a variety of their own peculiar speciality. And to interview the symbols of a mathematical logic for the prime definition of a fundamental objectivity, like force, is to revert to a barren source of knowledge, by an obsolete process in philosophy, and bar all progress in anything but abstract technique.

A COMPARISON OF BRITISH AND METRIC MEASURES FOR ENGINEERING PURPOSES.

By ARTHUR HAMILTON-SMYTHE, B.A., M. Inst. C.E.

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II.

Mr. W. H. Preece said he happened to be a member of the Committee of the British Association which had succeeded in establishing a system of electrical units based on the metrical system, and that system was now in use by every nation throughout the world, so that electricians in every country could speak amongst themselves in a language understood by all. There was not a laborer in any electrical engineering establishment who did not speak in accordance with a metric system which was completely understood. The advantages of the metric system were not to be met by ridicule. It would be an easy thing to turn the British system into ridicule. It might be said, for example, that Englishmen were so consistent that they measured the depth of the ocean by fathoms, and the height of mountains by feet; they measured their own height by feet, and the height of their horses by hands; they measured the surface of the land in miles of 1,760 yards, and the surface of the sea in miles of 2,025 yards; ale and beer were measured by barrels, kilderkins, hogsheads, pipes, and the like; they weighed their fuel in chaldrons, tons, or loads, according as they used coke, coal,

or wood; then there was Troy weight, avoirdupois, and apothecaries' weight, with scruples, ounces, pounds, and so on. But ridicule would not convert any one. The only valid objection raised to the decimal system was that it could not be divided by one third. For two years he had been trying to find out any circumstances that had necessitated his dividing by one-third, and the only thing that had come under his notice was the bill of his lawyer, who divided the pound by one-third and charged 6s. 8d. It had been stated that the metric system was invented by the French; but with reference to the decimal system, it was worth knowing that on the 14th of November, 1783, before the system was proposed in France, James Watt wrote a letter to Mr. Kirwan, in which he fully developed a decimal system based upon the pound.* He agreed with previous speakers that the decimal system and the metric system should be kept separate. The metric system was a system of measurements based on uniformity and simplicity, a system that would bring the whole civilized world together under one roof, and enable men to speak to each other in one language. He did not like to prophecy, but he thought the time was not far distant when the engineering profession would, like one of its branches, the electrical, seize upon the decimal and metric system as a labor-saving machine. It was a mistake to say that the metric system was not a binary system; it was essentially a binary system. Reference had been made by other speakers to the use in France of half a kilogram and half a millimeter. All men reasoned by experience. The man who had been accustomed all his life to the one-foot rule could not understand the meter, just as the man who had been used to the meter could not understand the one-foot rule. The English system had the merit of being ancient. It was established in 1266 by Henry III., who ordered that for the future 1 dwt. should be thirty-two grains of wheat, and that 20 dwt. should be 1 oz., and 12 oz. 1 lb. Edward II., in 1324, ordered that three barleycorns should be 1 inch, and 12 inches 1 foot. The British system, therefore, rested upon the scientific basis of the weight of a grain of wheat and the dimensions of a barleycorn.

Mr. Hamilton-Smythe, in reply upon the discussion, said he regretted that criticisms of the particular calculation in metric, and in decimalized British measures which he had, perhaps somewhat unadvisedly, selected, appeared to have diverted a good deal of attention from the broader question of the general comparative merits of British and metric measures for engineering purposes. By ingenious sleights of mind, when calculating British measures by vulgar fractions, and by great proficiency in mental arithmetic, some English engineers were able to evolve results in engineering calculations which excited the admiration of simpler arithmeticians. The capacity for such feats had no doubt been fostered, as was suggested by Mr. De Salis, by long practice and experience in battling with the complexities of British measures; and the difficulties which English boys had to contend with in learning British arithmetic and mensuration had, in many cases, developed mathematical capacities similar to those literary capacities which were supposed to have been developed by long and arduous studies of ancient Greek literature. Literary men, however, were able to escape from the Greek particles and irregular verbs after leaving a school or college; whereas, British engineers could never evade the wholesome discipline of those rather irksome educators, the compound rules of arithmetic and the British tables of weights and measures. It had once been urged that the introduction of machinery would destroy the individual capacity of the handicraftsman. Possibly this had been the case, yet the general result of the introduction of labor-saving machinery could hardly be alleged to have been unsatisfactory, and it might be asked whether the time now devoted to mastering the complexities of British arithmetic and mensuration might not be more usefully employed in overcoming technical difficulties, the study of which might develop equal mental capacities with more directly remunerative results. As time went on, and England became more fully equipped with engineering works, British engineers would probably find themselves more dependent on foreign work, and brought

* "The Origin and Progress of the Mechanical Inventions of James Watt," by J. P. Muirhead, vol. ii., p 179.

more into competition with foreign engineers in securing it. A considerable portion of this work would lie in countries using the metric system, so that familiarity with the system which, to quote Mr. De Salis, "appeared to commend itself to their clients," would probably be of ever-increasing importance to British engineers; while, if they were always to be burdening their memories with a double set of standard engineering dimensions and quantities, for use alternately at home and abroad, they would carry a load that would handicap the brains of most in the race for foreign work. If engineers wanted to get the full advantage out of a system of measures, they must accustom themselves to think in it. Mr. De Salis had deprecated any alteration in the size of the brick generally used in England; but bricks measuring 22 centimeters by 11 centimeters by 7.5 centimeters, would probably bond in sufficiently well with standard English brickwork. At any rate, the quality of the compound brickwork would depend, most likely, more upon other considerations than on the minute difference in size between the two kinds of bricks. He agreed with Mr. Walton Williams that a leveling-staff divided into separate meters was most inconvenient, and productive of error when read through a level at short distances; but if Mr. Williams would try the leveling-staff mentioned in the Paper, divided throughout into decimeters and centimeters only, omitting meter marks, his difficulty would disappear. Such leveling-staves had long been in common use, and he had used one in Austria for several years. The metric system afforded an ample supply of drawing scales to suit the convenience of the draftsman and the sizes of his drawing paper; but it was hardly to be expected that conveniently finite metric scale would coincide exactly with the various existing British scales, which had been adopted on account of their finite relation to the various British measures. If such had happened to be the case, one of the reasons for discarding British measures in favor of metric measures for the sake of facility for international comparison of maps and drawings would not exist. He thought no practical disadvantage would arise, as regarded the convenient size of drawings, if scales of $\frac{1}{50}$ and $\frac{1}{100}$ were used with metric measures instead of scales of $\frac{1}{48}$ and $\frac{1}{96}$ with British measures. It should be remembered that metric scales were not meant to be used to represent feet and inches, and conversely that British scales were not suited to represent meters and their decimal submultiples. As Mr. Thelwall had objected to the waste of figures involved by writing 200 millimeters and 13,000 kilograms, he might economize in them by writing the synonyms 2 decimeters and 13 tonnes. Mr. Airy seemed to have implied that the feet sometimes used in France and Norway were of the same length as the British foot; whereas he understood these feet differed about 1 per cent. and 3 per cent. respectively from the British foot, and if so would be of little value for international purposes. The adoption by Russia of the British foot was an argument, though of limited value, in favor of British measures which had been overlooked in the paper; but it was probably merely the correction of the length of a foot that had been used in Russia ever since Peter the Great carried home an English carpenters' foot-rule, and imposed it on his subjects as a standard measure. Mr. Hanson had joined Mr. Walton Williams in a defence of the $\frac{3}{16}$-inch to a foot scale; but the advantage claimed for it of suiting the existing divisions on a carpenters' rule would disappear if the carpenters' rule became metric, and the carpenter would then find that the natural scales of $\frac{1}{50}$ and $\frac{1}{100}$ would suit the metric rule quite as well. So far as he was aware, there had never been any question of the metric system affecting the British standard railway gauge, which was practically the same as the standard railway gauges of most countries using the metric system; and he thought Mr. Hanson was hardly justified in assuming that if the metric system were introduced into England, bricks would necessarily be made $\frac{1}{4}$ meter long. He regretted that Mr. Hanson had not stated what the young artisans in France happened to be measuring with a two-foot rule. If, for instance, they were measuring British-made machinery, it might be more convenient to use a two-foot rule for that purpose, on account of the various dimensions of what they were measuring having been

constructed to finite British measurements. Many of the inconveniences attributed to the metric system would be found to vanish on investigation. If computers preferred expressing metric measures in vulgar, instead of in decimal fractions, they were at liberty to do so, nor was there anything to prevent a man speaking of ¼ meter instead of 25 centimeters. It had been urged by Mr. Vernon-Harcourt that if metric measures were allowed to be used in Parliament, both witnesses and the legal profession would be much puzzled. He doubted, however, whether the average witness could be as easily puzzled with metric measures after a short experience of them, as he could always be with British measures, while the members of the Parliamentary Bar had certainly sufficient intelligence to enable them to master the metric system in addition to their briefs. He was struck with Mr. Barry's quotation from Lord Melbourne. Lord Melbourne, having attained to great eminence in his own line of life, apparently became disposed to think that changes must necessarily be for the worse. But there were many things, including the methods of measuring used in England, which had not yet been brought to annular perfection, and it appeared, from the remarks of Mr. Preece and others, there were some eminent members of the Institution of Civil Engineers who thought our time-honored British measures for engineering purposes might be improved upon with advantage. Almost every large general question resolved itself into a choice between evils, or into a balance of advantages, and the question between British and metric measures was no exception. He contended that, on the whole, the net balance of advantages was so much in favor of the metric system, that all impediments to its use in England should be removed as far as possible; and that it would be to the ultimate interest of English engineers to begin to make use of it for professional purposes. He believed continental experience, as he had pointed out in the paper, had proved this to be much more easy than many members would have been disposed to think, and the temporary translating of the comparatively few measurements which specially concerned the outside public in the plans of engineering schemes, had not been found so laborious as might have been supposed. In Ireland, land purchased for engineering works had to be computed in Irish land measures, the conversions into which were at least as troublesome as the conversion of hectares into statute acres. Sub-contracts for fencing, walling, and even some other kinds of masonry, were in Ireland set in Irish lineal perches, and various local measures had to be used there for purchasing lime, road-metaling, and some other engineering materials. Any one taking the trouble to investigate the approximate relations existing between metric and British measures, would find rules for approximate and rapid interconversion, which were at least as easily remembered and applied as the many ingenious mnemonics so freely used for abridging British fractional arithmetic and mensuration. Take for instances of the former—

10 lineal meters approximately
equal to 11 lineal yards.
10 square meters approximately
equal to 12 square yards.
10 cubic meters approximately
equal to 13 cubic yards.

When once, however, metric measures had supplanted British standard and local measures, all such approximate and other converters would be discarded and give place to more remunerative engineering knowledge.

Sir. Frederick Bramwell, President, said he wished, before the close of the meeting, to ask the indulgence of the members for 0.0833, or it might be 0.1666 of an hour. What was the object of a system of arithmetic? He supposed that it was to be able to make calculations in the easiest manner, and to arrive at sufficiently accurate results. The question, therefore, was whether the system in use in England, or that in use on the Continent, was the more likely to satisfy these conditions. He felt inclined to say, from many years' consideration and experience, that the English system was the more likely to give this satisfaction. Of course a mere statement of opinion, if unsupported by facts, was of no value, and he therefore desired to give one or two reasons for the faith that was in him. He wished to be allowed to use the decimal system with English weights and measures when he liked, and to use vulgar

fractions when he liked. At the present time the metric system was permissive in England, so that any one could use it when he pleased; but he presumed that the object of the author was to have an Act—a compulsory Act—which should forbid the use of the present English weights and measures. What would be thought by the advocates of the metric system, if those who preferred English weights and measures were to introduce a bill for the purpose of prohibiting the metric and the decimal method. They would, no doubt, look upon such a measure as being very wrong and improper, and he must be permitted, on the other hand, to regard the introduction of a bill to compel the use of the metric system as being equally wrong and improper. His hearers must not suppose that the advocates of the metric system were not amenable to the charge of seeking to make the continued use of the existing system a crime. The bill brought in by Messrs. Ewart, Bazley, Baines, Smith and Graves on the 24th of February, 1868, contained the following penal clauses:

"10. From and after the expiration of ———years from the passing of this Act, the Imperial and all local or customary weights and measures shall be abolished, and every person who shall sell by any denomination of weights and measures other than those of the standard metric weights and measures, or such decimal multiples or decimal parts thereof as are authorized by this Act, shall on conviction be liable to a penalty not exceeding the sum of forty shillings for every such sale.

"11. From and after the expiration of ———years after the passing of this Act, if any person or persons shall print, or if the clerk of any market or other person shall make any return, price list, price current, or any journal or other paper containing price list or price current, in which the denomination of weights and measures quoted or referred to shall denote or imply a greater or less weight or measure than is denoted or implied by the same denomination of the metric weights and measures under and according to the provisions of this Act, such person or persons or clerk of the market shall forfeit and pay any sum not exceeding ten shillings for every copy of every such return, price list, price current, journal, or other paper which he or they shall publish."

There must indeed be an extreme superiority of one system over the other, to justify an enactment that would cause a man to be considered a breaker of the law and liable to penalty simply because he chose to make his calculations by the old method instead of by the new one. All that he asked was, that liberty should be left to people to make their own selection, and he thought if that liberty were continued it would be easy to foretell the result. The permission to use the metric system as a legal measure had existed in England for some years, and in the United States for a still greater number of years, but it had not been adopted; whereupon the advocates of the metric system, not content with leaving it to the selection of the people to use that which was most convenient, wanted to force their particular mode upon them by means of penalties. To come now to a consideration of the relative advantages and disadvantages of the two systems, What were the facts? The author had spoken of "stones," "drams," "scruples," and so on; but such measures were not used by engineers. He had ridiculed the scale of three-sixteenths of an inch, but, as had already been pointed out, that was one sixty-fourth of a foot, and he believed that his mind was as capable of grasping the idea of one thing being sixty-four times the size of another as it was of being impressed by the notion that the relative dimensions of two objects were 1 and 100. The author had brought forward a pair of calculations to illustrate the general superiority of the decimal-metric system above the existing English system; but in one sense, and in one sense only, was this calculation that had been placed before them typical of the metrical and decimal system—in the enlarged copy on the wall the decimal point appeared in the wrong place, as it always did. Some years ago he went into the workshops of the Paris and Lyons railway, where he was shown a drawing of a locomotive, with a variable blast-pipe, and he asked what was the maximum and what the minimum area. One of the engineers took a sheet of foolscap, covered it with figures from top

to bottom, and then gave him a dimension rather bigger than that of the cylinder. Sir Frederick Bramwell had a two-foot rule in his pocket, and, finding that the drawing was made on the scale of one tenth, he applied the English inch tenth, and so got out the area and translated it into French measures, which he did in one-fifth part of the time occupied by the man figuring on the paper. He did not displace the decimal point, because he had not got one. Reverting to the calculations on the wall, he wished to show how utterly misleading they were. The author had placed before the members two comparative calculations, employed to ascertain the weight in tons and decimals of tons of the water contained in a given sized vessel. In consequence of the bulk of water representing the weight in French measures he was enabled to stop in his calculation on arriving at the cubic contents, and to say, "The whole thing is done; there is the weight of the water; but if you do it in English measurements you will have all these additional figures to use before you can get the weight of the water." Assume for the moment the difference in the length of the two calculations existed, what did it prove as regarded the general question? Nothing whatever. To what did it apply? To fresh water at a particular temperature, and to nothing else. There was no other liquid on the face of the earth, from ether to mercury, for which it would be true. It was not true for salt water, nor would it even do for fresh water at a different temperature. In any other case a multiplier must be used to get the weight, which would make the metric calculation as complex as the author's English example. But who but one whose mind was warped by the metric system would have thought of turning inches into decimals of feet prior to calculation? Would not anyone else have worked the sum thus?: The annexed sum showed how any one not saturated with decimals dealing with 10 feet 6 inches by 6 feet 2 inches by 1 foot 1 inch would have treated it. There, in twenty-five figures, was the answer, as regarded the cubic contents, while the metric system, to reach the same point, had needed thirty-five figures. Mr. Percy Fowler had spoken, to his great astonishment, of the way in which workmen in Spain used

10	6	
6	2	
63	0	
1	9	
64	9	
1	1	
64	9	
5	4	9
70	1	9

the metric system, and made calculations which persons of the same class could not make in England with the ordinary English measures. He had never traveled in Spain, but he had traveled in France, Italy and Germany, and had made it his business to ascertain what the facts were with regard to the powers of the people to do anything in the way of mental arithmetic, and he said unhesitatingly that in those three countries it was the rarest possible thing to meet a person who could make a mental calculation, not because they were wanting in intelligence or ability, for they were quite equal to the English in those respects, but because they dealt with a system so cumbrous that it absolutely precluded mental calculation. Let any one go to a French railway station, and ask for three tickets from A to B, and it would generally be found that the man (or, as was commonly the case, a woman, with a man to look after her) could not tell the amount without taking a piece of chalk or a pencil and making a calculation. The clerk would have no more idea of what three times the single fare was than a child would have. Compare such a person's power of calculation with that of an English butcher's wife or daughter who was in the habit of dealing with pounds and ounces and pence. Let him test the question nearer home. Could many of those present mentally square 3.25? He believed very few. But there was no difficulty in squaring $3\frac{1}{4} = 10\frac{9}{16}$. $10\frac{9}{16}$ was a sum easily appreciated and easily expressed, while in 10.5625, the decimal equivalent of the vulgar fraction was much more cumbrous, and he ventured to think did not form any impression on the mind, except that it was a little more than one-half. Again, which of them could square 4.125? That to the majority would be almost impossible, but with $4\frac{1}{8}$ there was no difficulty.

$17\frac{1}{64}$ compared with 17.015625. It appeared to him that a system like ours, which enabled mental calculations to be made rapidly and accurately was enough for all practical purposes. It did not lie in the mouth of decimalists to insist upon absolute accuracy, because they were content with approximation, and must of necessity be so in many cases. With regard to the statement as to the meter being the ten-millionth part of a quadrant of the meridian, Mr. Hawksley had fully exposed this in his presidential address, to which he would refer the members.* The whole thing had been a failure, and Frenchmen themselves did not use the notation, nor did they use the unit of weight. It had been intended, if it were desired to express the thousandth part of a meter, it should be done by writing the word meter, and putting below it a decimal point followed by two 0's and a 1, thus $\overset{\text{Meter}}{0.001}$; but what was the fact? A millimeter was expressed by m/m.* So with the kilogram. It was intended that should be the unit, and the lesser divisions were to be indicated by a decimal point and the regular number of ciphers; but the fact was the unit was too large, and accordingly the French bought and sold by the half-kilogram, and they called it the $\frac{1}{2}$ kilo, and not the 0.500 kilo, or even 500 grams, these modes were too roundabout as compared with "half a kilogram," and humanity would say half a kilogram in spite of penalties. Centimes, again, were often replaced by sous. The "tonne," also, was not a part of the metric system, but was an invention to cure a failure in the system. People did not and would not deal with thousands of figures when they could adopt another mode of expressing the number compendiously by a single figure. As regards the facilities afforded by English weights and measures, he desired to be permitted to refer to the numberless short cuts in mental calculations given by the present system. How much they would have to forego if they were forbidden to say that plate-iron was 5 lbs. to the foot super, and cast-iron 4 inches to the pound, adding $\frac{1}{20}$ to make the necessary correction. Round-iron, by the present system could have its weight calculated in a moment, squaring the diameter in eighths and dividing by twenty-five, or multiplying by four, gave the weight in lbs. per foot at once; for example, $1\frac{3}{8}=11$, $11^2=121$, $121\times4=484$ lbs. per foot. Water had been referred to as an instance of the wonderful use of the metric system. He would take a water illustration of a simple calculation by the English system. If engineers wanted to know how much a pump would lift, what did they do? They squared the diameter and multiplied by the stroke of the piston in yards, and at once obtained the amount in lbs., as every yard upon the circular inch was a pound. Taking an 8-inch pump, the square was 64; it was making 10 yards a minute—64 gallons a minute—there they had it in a moment. Another instance, 1 inch of rain to the acre was 100 tons, or, to be more accurate, 101 tons. In the United States, where, as here, they had the option to use legally the metric system, they had not used it. Mr. Sellers, one of the best authorities, had said that the thing was not fit to be used. He would refer the Members to one who was not a bad engineering authority, Rankine. He would read to them the last verse of Rankine's song in praise of the three-foot rule:

"Here's a health to every learned man that goes by common sense;
And would not plague the workman on any vain pretence;
But as for those philanthropists who'd send us back to school,
Oh, bless their eyes, if ever they tries to put down the three-foot rule."

He would also read an extract from a speech of Mr. Beresford Hope on the same subject, made in the debate on Mr. Ewart's Bill:

"Decimalization is a process of calculation for the benefit of the calculator. Metricalization is not a process, but a system of measures, so called from its unit or base, which happens accidentally to be facilitated by the ease with which its details may be worked out through means of the decimal notation. The metrical system in itself is an abtruse and philosophic one, founded upon the fancy of some French men of science at the time of the Revolution, who adopted as the starting-point of the system the measurement of the earth's circumference, and by the way of a unit, measured the 10,000,000th part of a quadrant of a

* Minutes of Proceedings Inst. C. E., vol. xxxiii. p. 342.

meridian through Paris (about $39\frac{5}{12}$ in.) which they termed a 'meter.' No doubt those multiples and aliquot parts of the meter which form the French measures of length are adjusted to meet the decimal system, as are also the measures of area, capacity and weight, which are by a further process built upon the meter. But decimal notation is equally applicable for the man who finds that it helps his calculations whenever he has to work out his sum in our old weights and measures; for decimals are really not a system, but, as I said, a process for easily reaching a certain practical result, like logarithms or algebraical symbols. I grant all the advantages which their friends urge in behalf of decimals for the purpose of calculation; but it requires no Act of Parliament to enable those who appreciate them to make their own calculations by way of decimals. Least of all, is legislation needed for the merchant princes—the men of enormous means and gigantic transactions—whose advocate my hon. friend the Member for Liverpool (Mr. Graves) has made himself. They have but to keep a calculating clerk—an employé whose one duty is to manipulate the decimals—and they have got what they want. The sufferers will be the little people—the small buyers and sellers, the hucksters and the marketers—who will be compelled under the penalties of a compulsory Act of Parliament, to learn and to use a system which is, in its outward type, as non-natural as it is novel. I will, in order to prove my point, take the most familiar instance, and show that although a great deal has been said about the advantages of the French subdivisions, yet, after all, our subdivisions are more natural for the ordinary purposes of life. If a boy has to divide an apple, does he ever think anything about the circumference of the earth and its aliquot parts, or about the decimal system and its unrivaled facilities for calculation? No; but he takes his apple, and cuts it into two parts if he wants to halve it, and those halves into quarters if he wants to make four parts of it. In the same way, if a housewife has to cut up the loaf for her family, she divides it into two, into four, into eight, or into sixteen parts, and the sixteen people share their bread naturally. Supposing the loaf to weigh originally a pound, each of these sixteen divisions comes out an ounce. Such is the *rationale* of our system of measuring—binary system so-called—founded on continual halving, and proved, by the common sense of mankind, before the great era of enlightenment inaugurated in 1789, to be the most convenient and natural one. But I may be told—Halve away, but then express your halvings in decimals. This is very easy for the merchant prince to do when he is totting up his large transactions in 'centals,' or for the Chancellor of the Exchequer when dealing with a nation's finances; but how will it suit the little transactions of daily life? I come back to my loaf. How are ordinary people to represent halves and quarters by decimal points? The symbol of a half is the figure 'five,' with a dot to its left hand; the symbol of half that quantity, that is of a quarter, is the sum twenty-five, also with a dot to its left hand. Arithmeticians understand how this can come about, and the symbols have grown natural in their eyes; but in what—even the most infinitesimal—degree do they tell their own story to the unlearned? What palpable relations towards each other can be disentangled out of these most frequently recurring symbols? What is there in the nature of things to show that the dotted five means a half, and the dotted twenty-five a half of that half, and a quarter of the 'one,' with no dot on either side, which stands for unity? Decimal notation is then, after all, as I have been arguing, a process, and not a system. It is a process good for the schools, and good for the bustling counting-house and the large sum, but the poor man would be completely thrown out if he had to employ—under penal legislation, too—decimal points for the purpose of measuring his little purchases by halves and quarters. With permissive means, such as now exist, the system will come in where it is wanted, and be kept out where it is not wanted; but under a compulsory enactment it will intrude itself everywhere, and show itself in its real colors as nothing less than a public nuisance. But the more we examine the bill of the hon. Member of Dumfries, the more inapplicable do its provisions seem for the purposes of practical life. I have touched upon the principle of the metric system, let me now call the attention of

the House to the language in which (after the French model) it is proposed to clothe that system. The new unit of weight is to be the 'gram' or 'gramme,' which is attained by providing a square vessel, whose capacity is the cube of the hundredth part of a meter (centimeter, to wit), and then weighing the amount of water which it will hold at a certain temperature. One-tenth part of this gram is to be a de*ci*gram, and ten times a gram is to be a de*ka*gram, for the reformers decreed that aliquot parts were to be named after the Latin, and multiples after the Greek numerals. How in the name of common sense can we make poor people understand that because there are the letters 'ci' in the one word it means the tenth of a gram, and that because there are the letters 'ka' in the other it means 10 grams, or 100 decigrams? My hon. friends the Member for Dumfries and the Member for Liverpool come to this House representing great commercial transactions; but I stand up for the poor man. Only imagine an honest housewife going into a shop and asking for a decigram of pepper, and a dekagram of tea; imagine, too, the milkmaid selling her fluid by the liter. The Member for Liverpool is a kind-hearted man; is he then prepared, with all the stringent force of a penal statute, to enact that, when one of his youthful constituents may desire to effect a commercial transaction in a manufacture for which one portion of that great borough is famous, he should be bound to go to the shop and tender his 'dime' for three decigrams of Everton toffee? Fancy the farmer who has been accustomed ever since he entered on his farm to cultivate the 'ten' or 'twelve-acre field,' having to consult the steward about liming the seventeen *are* field, or be a criminal and a contemner of the laws of his country. Fancy the bumpkin who was prepared to boast that he was within a decimeter of catching the fox as he crept through a gap about a dekameter from the white gate. If the theorists and the men of wealth—men of brains, it may be, but as certainly men of self-assurance—have worked out this system for themselves, there are poor men, who form the majority of mankind, for whom it will never answer, and there are men of brains at least equal who are decidedly opposed to its adoption. Is it not possible that our present system is not only quite as convenient and useful as the metric system, but a little more philosophical also? Why should a standard founded on the quadrant of the earth's circumference passing through the meridian of Paris be a better one than ours? No doubt it looks very solemn, from the grand nomenclature with which it is propped, but all those odd names for the French weights and measures were adopted at the first heat of the great Revolution, when the pedantic aping after ancient Greek and Latin terms led to their being applied to everything novel and French—from the scanty proportions of a lady's dress to the most intricate principles of jurisprudence and moral philosophy. Moreover, they have taken root in nations whose vernacular languages are themselves derived from the old classical tongues. May it not, I repeat, be just possib e that our unit is as good as that of the French, even upon the most abstract grounds."*

Mr. Beresford Hope, having spoken in that common-sense way, had quoted a letter from Sir John Herschel, which was very well worth reading, but he would not detain the members with it. He would only refer to one other matter, and that was the coinage. It had been said, you may decimalize the pound without difficulty. This was one part of the case which had not been considered. Once let the pound be decimalized, and there would be an end of the guinea. He did not know whether there were any physicians or barristers present, but if there were he would remind them that by the new system they would lose 0.047619 of their incomes—in other words $\frac{1}{21}$. When the pound was decimalized no one would be prepared to pay the next division, which would be $\frac{1}{10}$ instead of $\frac{1}{20}$. In conclusion, he would tell them of a ready way which he always used for mental calculation when turning millimeters into inches. Mr. Rapier had said, multiply by 4, but this needed a correction, which was, deduct $\frac{1}{64}$ of the product; this would give an answer as near the truth as the 3 .. $3\frac{3}{8}$ commonly taken; for example, 16 millimeters $\times 4 = 64 - \frac{1}{64} = 63 = 6.3$ inches.

* Hansard's Parliamentary Debates, vol. cxcii., May 13th, 1868.

CORRESPONDENCE.

Mr. A. Barclay remarked that there was a convenient mode of graduating and figuring a rule which was applicable to any arbitrary unit and subdivision. In the ordinary fitters' and joiners' English two-foot rule, folded in the middle on a compass-joint, the inches, figured on each face from left to right, 1 to 24, were usually divided into $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, and $\frac{1}{16}$. If any work had to be centered, the unit end of the rule was made to correspond with one limit, and the reading was taken at the other limit (say $21\frac{3}{4}\,\frac{1}{16}$ inches bare), it was then necessary, mentally, to halve this quantity, represented by two numerals, two fractions, and the difference to the nearest division, and having by this process obtained the new half quantity ($10\frac{7}{8}\,\frac{1}{32}$ inches, scant, which had also two numerals, two fractions and a difference), to note the corresponding reading on the rule, and against it make a trial center mark, which was very likely to require adjustment. Next, the accuracy of its position must be checked by an equal half measurement to the other limit. The rule which he preferred had the usual divisions and figures, and also on one face next the outer edge were two scales of half inches, figured 1 to 24, from the joint to each end, the half inches having the same number of subdivisions as the scale of inches. With this rule, having ascertained the cross measure (say, as before, $21\frac{3}{4}\,\frac{1}{16}$ inches bare), and found the corresponding figures and divisions on the two half-inch scales, they would, when laid on the work, exactly coincide with the two opposite limits, and the center would be marked off with confidence at the joint zero. It was only necessary to remember the first measurement as a comparative length, calculation was unnecessary, and halving a measurement by this method of substituting the appropriate scales was accomplished with accuracy and dispatch. The Japanese artizans, now at work at the Exhibition at South Kensington, used a bamboo rule of native make, like a drawing scale, the unit being the English foot divided into 10, 100, $1\frac{0}{0}0$.

Mr. H. Bauerman, as the result of long experience in the use of various metrological systems in different countries, was unable to agree with the author's conclusions as to the desirability of internationalizing the metrical system to the exclusion of all others. He thought that the weights and measures used at any particular time by any people fairly corresponded to their local and temporary necessities, and as new necessities arose, they would be met by the adoption of new, or by the modification of old, units. Take, for instance, the progress in the use of the ton as a business unit. For many centuries commercial requirements were satisfied by the hundredweight or quintal, and indeed the latter, until lately, if not now, did survive in the Newfoundland and Labrador cod fisheries. Increase in the scale of operations, however, led to the adoption of the twenty-fold larger unit, first in England, and at longer intervals in foreign countries. Strangely enough in Germany, where the cwt. unit lingered longest, the ton had been supplemented by a ten-fold larger unit, the wagon, corresponding to the contents of a ten-ton railway wagon, which was now commonly used in the coal and iron districts of the Rhineland. These were examples of change of use due to commercial necessities, and which had been effected without inconvenience; but it would be very different to attempt to impose an entirely new metrological system upon a people without regard to local usages. Those who advocated the universality of the metric system seemed to consider arithmetical convenience as synonymous with convenience of every kind, which was not necessarily true. For instance, when a large number of similar articles had to be divided into small packages, there was a distinct saving of packing materials by making them up in twelves rather than in tens. Here the convenience of the packer was in sharp contrast to that of the computer, so that while the latter might prefer to consider twelve as 1.2 decade, the former would regard ten as five-sixths of a dozen, and it would only depend upon the relative strength of the two parties which view should prevail, if one were to be used to the exclusion of the other. The author's arithmetical illustration did not appear to have much bearing upon the question, for although he had used a large number of figures to arrive at an incorrect result in determining the contents of the English tank, as contrasted with a smaller

number required for the determination of the equivalent metrical volume, the former was due to the use of incorrect factors, and an inappropriate arithmetical method, which, again, was conditioned by the assumption that civilized arithmetic must necessarily be decimal. In other words, he preferred multiplying by 0.167, which was inexact, to dividing by 6, which was right. The author's suggestion that English engineers would be precluded from using foreign technical literature, because, in the latter, dimensions were, as a rule, expressed in millimeters, did not seem to accord with the experience of the Institution, which for some years past had supplied abstracts of the more important papers in foreign journals, in English, but in many cases with the original dimensions. These had been so far appreciated as to be largely reproduced in English and American journals when of special interest, which would scarcely have been done had the editors considered them as a matter unintelligible to their subscribers; and, in the case of one contributor, at any rate, such abstracts had led to correspondence sometimes of an inquiring and sometimes of a mildly controversial character, but in neither case had there been any misapprehension as to dimensions. The author's views as to decimalizing English money were also contrary to experience. The physical decimalization of the sovereign had been attempted in England by the issue of double shillings, which were intended to supersede half-crowns, and the coinage of the latter was discontinued for many years; but when they became scarce the public asked for a further supply, and got them, whereby the decimal system, pure and simple, was tacitly abandoned, but at the same time public convenience was enhanced by having the two kinds of coin instead of only one. The Latin Monetary convention, in its present form, was rather intended to reduce the circulation of a token coin, the franc of $\frac{835}{1000}$ fine, than to facilitate its international currency.

Mr. F. Briffault pointed out two instances that had come under his notice in connection with two foreign waterworks where the decimal and metric systems alone were used. The Brazilian Government had been supplied with 95,000 tons of pipes and connections from this country for the waterworks of Rio de Janeiro, this quantity being divided between four manufactories. The castings were all weighed in kilograms, the weighing machines having been made expressly for the purpose. At first the men did not take kindly to the innovation, but at the end of a fortnight, after finding out the great saving of trouble thereby effected, they much preferred this system to the British. Having only to deal with one unit of weight, the kilogram, the numbers were at once read off the weighing machine, and at the conclusion of the day's work the columns were added up, and three figures pointed off to the left gave, of course, the total in metric tons. Only simple addition being necessary, the class of men entrusted with these operations could do their work without error, whereas, had the weight been of of several denominations, a hopeless muddle would have arisen. At the termination of the contract there was only a discrepancy in weight between the Government authorities and the contractors of under three tons. Considering the magnitude of the undertaking, such a result would scarcely have been attained had British measures been used. There had also been a great saving of time and money in clerks' salaries by the adoption of this system, in the two and a-half years employed on the work. In the case of the Constantinople waterworks, similar satisfactory results had been obtained by the use of the decimal system; but in Turkey the advantage of it over the British system was even more marked. The number of pipes arriving damaged and cracked in transport up country was very great; they had, consequently, to be cut on the spot in order to be utilized, and there was thus a great variety of of lengths to deal with. To weigh the pipes after cutting was impossible, for many reasons, so the reduced lengths were measured in meters and centimeters, and the maximum weight allowed per lineal meter being fixed, the two had but to be multiplied together, and the result was metric tons and decimal parts of the same. With feet and inches, and tons, cwts., qrs., and lbs., a vast amount of tedious work would have been necessary, with a doubtful result as to its correctness.

Mr. W. D. Chapman remarked that in

the *Standard* of the 16th of December, 1884, there appeared a leading article to the effect that a Dutch company could sell Dutch milk in London "at the price of 2¾d. per liter—or, in other words, at a little over 2d. per quart." On proceeding to check this calculation, he found that 2¾d. per liter was 3½d. per quart—a difference which completely disposed of the arguments in the *Standard*. He thought that many practical English farmers, interested, as he was, in English dairy farming, had been misled by having to assume the accuracy of the erroneous conversion of measures, and had needlessly admitted the hopelessness of competing with the Dutch company, simply because the want of an international language of measurement deceived them as to the facts of an enterprise affecting their business. This was merely one recent example of many cases where English business had been fettered and English business men deceived, in questions of international competition where measures were concerned.

Mr. John Craig observed that most of the advantages and disadvantages of substituting the metric system of weights and measures for the British system had, there was no doubt, been fully entered upon and discussed. He wished only briefly to add his experience of the working of both the above systems, and also of other systems in various countries. Great weight must be attached to the remark in the paper as to the position held by England and the United States of America in the manufacture of engineering machinery. It was generally admitted by engineers that any violent change of the measures now in use would be practically impossible, as far as this branch of mechanical engineering was concerned; besides, a strong feeling existed among many mechanical engineers against any change; they even asserted that British measures were, for their purposes, better than any others in use. No such feeling existed amongst civil engineers; nor would interests be affected in anything like the same degree if the metric system were at once generally adopted in connection with all field and office work undertaken by them, for which the consent of Parliament had to be obtained. I have never met a civil engineer who had used the metric system in field and office, on railway or other public work, who did not admit its great superiority to the British. If his experience agreed with that of the majority of the civil branch of the profession, he submitted that steps should be taken to have the metric system allowed, or enforced, on all public works; its use would thus become known, and gradually extend, and the way prepared for the time when the metric system, or other system based on somewhat similar principles, should be adopted by all English-speaking peoples. Literature, in connection with all branches of the profession, would soon adapt itself to a new order of things. It had been said, "To acquire a new language is to enter upon a new world." The advantages a British engineer would gain by a literature written in terms of an almost universal measure with which he was familiar, would be great, indeed. The advantages derived from the nomenclature of the metric system, although great were, he thought, of secondary importance; that was, if the nomenclature should in any way prove a bar to the system coming into general use. He would prove a benefactor to his country who would suggest some good plan whereby he could retain many familiar names for certain of the metric dimensions. The adoption of the metric system in India, on the Government and public works, so far as might be required for engineering purposes, would not cause much inconvenience, especially if time were given. In most, if not in all our colonies, the change would be hailed with joy, and would form a not unimportant link in the chain which was gradually being forged, to bind them in one mighty federation with the mother country. In some of our colonies its adoption would do away with the difficulty of introducing British measures amongst peoples under our rule, whose system of weights and measures might be as good as ours, and whose likes and dislikes were quite as strong. At the Cape, for example, all railways and public works were carried out by the use of British measures. Land was, however, surveyed, bought and sold by Dutch measurement. The colonial ton, of 2,000 lbs., and cwt., 100 lbs., could not, he thought, with advantage be replaced by the British ton and cwt. In

the markets of the country the measures were most confusing, and there was certainly great need for reform. To have a decimal system of coinage would no doubt facilitate the use and introduction generally of the metric system. The great convenience of a decimal system of coinage could only be fully realized by those of our countrymen who had resided for a time in a country where such a system was in use. He feared, however, the opposition to change would be very great. Time could not not be given for it to come gradually into use, and a change, when made, must be compulsory on all. He did not think our accounts would ever be kept in pounds sterling and mils. The pound sterling must always remain, but florins and cents could easily be substituted for shillings and pence, and the British penny might still remain a force in the land, in name and appearance. Accounts kept in the above way would effect a saving of 20 per cent. in the number of figures required on a page of a written-up ledger, to say nothing of the advantages in other ways.

Professor J. D. Everett agreed with the author in thinking that the adoption of the metric system of measures and weights would be a great benefit, by effecting a saving of a vast amount of useless labor. The case had, he thought, been fairly and clearly stated in the paper. He was glad to find one point insisted on which had often been overlooked, namely the facility which a decimal system afforded for logarithmic calculation. The remark must be extended not only to the use of tables of logarithms, but to the use of logarithmic scales, such as the slide rule in its ordinary form, or in the circular form, or in the parallel-column form employed in his "Proportion Table," or in the helical form which, in Professor Fuller's "Spiral Slide Rule," admitted of working to five places of figures. He concurred in the importance of speedily decimalizing the British money reckoning and coinage. Any partial decimalization of the coinage, not carried far enough to admit of keeping accounts decimally, was of no advantage. In his opinion the arrangement that would least disturb existing prices would be to decimalize upwards from the farthing, because every sum of money, according to present reckoning, would have an exact equivalent in the new reckoning, whereas the present penny and halfpenny would have no exact equivalents in a system decimalized downwards from the pound sterling. But a minimum of disturbance was not the only consideration.

He was in favor of neither of the systems above compared, but had for many years held the opinion that the best system to adopt would be the American system of dollars and cents. When traveling in America many years ago he was greatly struck, not only with the facility afforded for calculation, but with the extreme convenience of the coinage. An abundance of change for all sorts of purchases could be carried without burdening the pocket, and though a stranger, he could count his change with as much ease as in his own country, and with much more ease than in France, notwithstanding that the coinage there was likewise decimal. He claimed that the American system was more economical of space in account books than either the French system or a decimalized system founded on the pound sterling. It started from the cent (about a halfpenny) as the unit in the right hand column. A decimalized system, founded on the pound must start from the mil as the unit in the right-hand column; for the hundredth of a pound (2½d., nearly) was too large for less sums to be ignored. The pound would therefore be the unit in the fourth column from the right hand, and space must be left for three columns to the right of it. In the American system only two columns were wanted to the right of the dollar column, so that the unit in the fourth column from the right hand was ten dollars. This was rather more than two pounds sterling, and thus with a given number of columns twice as large a sum could be expressed in the American system as in the pound and mil system. The disturbance of prices would be much less than in the pound and mil system, for 100 cents were 4s. 1½d., and 100 halfpence were 4s. 2d. The difference between a cent and a halfpenny was, therefore 1 part in 100, whereas the difference between a mil and a farthing was 4 parts in 100. He thought the importance of adhering to the pound sterling

in its exactitude had been greatly exaggerated, and he believed that no confusion would be introduced in foreign relations by making payments in five-dollar gold pieces, each worth £1 0s. 7½d., instead of in sovereigns, as hitherto.

A SUGGESTION IN SEWAGE PURIFICATION.

From "The Builder."

An experiment has recently been made by a scientific man at Buxton, which may possibly exert an important influence with regard to the disposal of sewage. Nor is it a theoretic discovery alone. Works have been erected, at the cost of £4,000, for the treatment of the sewage of Buxton (which varies in quantity from 200,000 to 1,000,000 gallons per diem), by the process to be described; and on the 9th of April the Buxton Sewage Works at Ashwood Dale were formally opened, and the occasion was duly celebrated by a public dinner.

The Rivers Pollution Commissioners have for some time past insisted that measures should be taken by the Local Board of Buxton to rid the Wye of the poisonous contents of the drains. In the case of a town chiefly known as a health resort, the subject assumed even more than usual importance, and a deputation of the Local Board took the wise step of visiting various sewage works, of which the principles had been recommended for their adoption. They went to Birmingham, where they found a sewage farm, and also a long series of tanks, and pronounced the system a failure. No fish would live in the water. They went to Bilston where the filtration system has been adopted. It was not, however, satisfactory. They also visited Coventry, where they saw the operation of the grinding machines on what is called the black-ash system, in which sulphuric acid is used. That they considered the best system they had seen. At Leamington they saw an immense sewage farm, for the effluent of which, produced at a cost of £1,100, £400 was received. This they regarded as a heavy loss, and a plan that would not answer at all. They visited Hertford, where phosphate of alumina is produced by the Phosphate Manure Company. That plan, they found, would cost 8d. or 9d. in the pound on the rates, if applied at Buxton. It does not appear from the report under what conditions Dr. Thresh was called in to advise; but this gentleman, in the course of experiments made from time to time as to the defecation of sewage, was led to examine the water flowing from an old coal pit, which ran to waste in the Wye, disfiguring the public gardens of Buxton by its course through them. This water is, in fact, a strong chalybeate spring. It contains salts of iron, aluminium, sodium, calcium, and magnesium, chiefly as sulphates, but a considerable portion of the iron is in the form of a carbonate held in solution by carbonic acid. From 1.2 to 2.4 grains of metallic iron occur in the gallon of this water.

The effect of this iron water on the sewage is remarkably prompt. The method adopted has been to mix with this natural water a certain proportion of milk of lime, and then to allow it to mix with two or three times its volume of sewage. By agitating gently, a flocculent precipitate forms and rapidly settles, leaving the supernatant fluid beautifully clear. Analyses made by Sir Henry Roscoe at Owens College are appended to the report of Dr. Thresh, from which we abstract these particulars.

Additional interest attaches to this method, from the fact that its principle is much the same as that first applied at Antwerp by Professor Bischof to the purification of the waters of the River Nethe by passing them through a mixture of spongy iron and gravel. The effect of iron in the destruction of organic matter suspended in water has thus not only been previously known, but the plan has been acted on, on a considerable scale, at the Antwerp Waterworks. In 1878 Mr. Bischof, as appears from the proceedings of the Royal Society, advocated the use of finely-divided or spongy

iron as a medium for the filtration of water. It was demonstrated, according to Dr. Frankland, that filtration through spongy iron destroyed much of the organic impurity, removed color, precipi tated finely-suspended solid matter, and, above all, destroyed the germs of putrefaction, and probably those of all kinds of epidemic disease. In 1879 a filtering apparatus was erected at Waelhem for the filtration of the water of the Nethe. A cast-iron tank, 18 ft. 6 in. square and 11 ft. deep, was coated at the bottom with cement concrete, covered with bricks on edge. On the bricks was laid a mixture of three parts of gravel with one part of spongy iron, 3 ft. thick, which was covered with 18 in. of fine sand from the Meuse. A second filter of a similar kind was placed at a lower level, so as to receive the water that had passed through the first. The results were so satisfactory that large works were undertaken, a description of which will be found in Vol. 72 of the Proceedings of the Institution of Civil Engineers. After eighteen months' experience it was stated at the Conference on Water Supply, held at the International Health Exhibition of 1884, by Mr. Anderson, M. Inst. C. E., that, as far as the purification of the water went, Professor Bischof's process left little to be desired, but that the working of the system had been costly.

The increasing demand for water rendering extension of these works necessary, Mr. Anderson, M. Inst. C. E., whose duty it became to advise the directors of the waterworks, made an experiment, suggested by Sir F. Abel, on the principle of passing iron through the water, instead of passing water over the spongy iron. Mr. Anderson constructed a revolving cylinder, 4 ft. 6 in. in diameter, and 5 ft. 6 in. long, which was furnished with inlet and outlet pipes, and also contained shelves or ledges for scooping up the iron used, raising it to the top of the cylinder by the rotary motion, and thus letting it fall through the water. Running water through this cylinder at 12 gallons per minute, which gives a contact of about 45 minutes, Mr. Anderson found the water to be very heavily charged with iron. At a flow of 30 gallons per minute, 1.20 grains of iron were dissolved per gallon, which was twelve times as much as the experience at Antwerp had shown to be necessary. At 60 gallons per minute 0.9 grain per gallon was dissolved. The result of the trial proving thus successful, the revolver was sent to Antwerp, fitted with large pipes, which sent 166 gallons per minute through it, and has been at work there ever since.

Thus the history of the application of iron to the purifiation of water comprises a number of independent experiments and discoveries, made by different men. More than twenty-five years ago Dr. Medlock and Mr. Quick, C. E., made a number of experiments on the purification of Thames water by metallic iron. The water of the river at Battersea was left in contact with iron wire and plates in a large tank, for twenty-four hours, and the improvement in quality was very marked. It is well known to naval officers (and has been mentioned in the columns of the *Builder*) that water stored in iron tanks that have been whitewashed inside becomes remarkably pure and sparkling, and that the vapidity of distilled water is removed by such storage. The Antwerp filters represent a further step in the same direction, although the propriety of the mixture of gravel with spongy iron has been called in question. Sir F. Abel's suggestion is marked by extreme elegance; as the weak point of all filters, that of becoming choked by their own action, is avoided by the very ingenious reversal of the usual method of producing contact with the metal. A no less original step has been taken by Dr. Thresh, and the review of the advance made in twenty-five years leads to the conclusion that much yet may be done towards the perfecting of the use of iron as a purifier of water.

Dr. Frankland, an unquestionable authority on the point, states that bacteria, which are indestructible by an atmosphere of pure oxygen, of carbonic acid, of nitrogen, of sulphurous acid and of cyanogen, are killed by a short contact with iron. As all the known forms of bacteria are affected in the same way, it is thus probable that all forms of bacterial life will be thus destroyed, and iron is the only known substance which produces this effect. Thus far, therefore, the progress of the application of iron may be taken as highly promising.

Several questions, however, remain for

solution. Mr. Anderson, Dr. Frankland, and other authorities, describe the action of iron as rapidly destructive of organic as well as of organized matter. In the case of the water of the Nethe, which is very impure, a contact of nine minutes is enough to dissolve 0.9 grain of metallic iron per gallon, and a contact of three minutes and a-half, which presumably will not dissolve much more than 0.3 grain per gallon, is found to be more than adequate to effect purification from organic suspended matter. Indeed, the waste of iron during thirty-three days is stated at 0.176 grains per gallon of water run through the cylinder. Dr. Thresh, however, speaks not so much of destruction as of precipitation, and even says that 100 parts of dry residue from the Buxton tanks contains fifty per cent. of organic matter. The difference is cardinal. But the Burbage chalybeate water contains, together with from 1.2 to 2.4 grains of iron per gallon, fifty grains of mixed crystalline sulphates. It is thus evident that when to this heavy proportion of mineral matter is added the milk of lime thrown in to hasten precipitation, the total amount of sludge formed must be far in excess of the inorganic elements of the sewage. It is in the mass of sludge that has in some way to be got rid of that the essential weakness of all precipitation processes lie. In the present case it is intended simply to cart away the sludge in a moist state, and put it on land belonging to the Board. This mode of disposal, as Dr. Thresh justly remarks, cannot go on for ever, and sale of the sludge for manure is evidently looked forward to as a resource. Indeed, the presence of so large a portion of organic matter in the dry residue is evidently regarded as increasing the stimulating value of the manure. Now, it is on the rock, or rather the quicksand, of the profitable disposal of the *residua* of city life that most of the schemes for sewage disposal have hitherto foundered. The true chemical value of the contents of sewage is so low as hardly ever, if at all, to be worth the cost of extraction. And the other materials, put in as precipitants, or added to "fortify" the manure, can usually be applied much better in their natural state to agriculture than as constituents of the heavy and unmanageable sludge. As far, then, as precipitation of organic matter takes the place of destruction, and as the addition of lime to the heavy chemical charge of the chalybeate water is required the results which may be expected from the Buxton process by no means come up to what we think may yet be secured from the brilliant discovery of Dr. Thresh.

The works recently opened have been constructed by Mr. Joseph Hague, A. M. Inst. C. E., the Town Surveyor of Buxton. The chalybeate water is conveyed by gravitation through earthenware tubes, with joints of jute, spun yarn, and cement, from a disused colliery at the foot of the Axe Edge Hills for a distance of over two miles. It then enters a tank at the rear of the liming rooms adjoining the works, which are situated between the river Wye and the Midland Railway, in Ashwood Dale. A series of flushing chambers, supplied with penstocks, is introduced at suitable places along the route, with a view to supplying the carts for street watering.

The liming and mixing rooms are erected over the River Wye, on a semi-circular stone arch, the liming-room floor being on a level with the adjoining highway, and connected with a siding on the Midland Railway by a tramway. One of Messrs. Bowes, Scott & Read's liming machines supplies a cistern of 800 gallons capacity, which is provided with an agitating apparatus. The machinery is driven by an overshot waterwheel, 16 ft. in diameter and 3 ft. wide, driven by water derived from the River Wye.

Outside the liming and machinery rooms are duplicate brick tanks, into which the main outfall sewer discharges. The tanks are furnished with wrought-iron screening-wagons, for the purpose of abstracting the solid and floating matter, which is estimated at 75 per cent. of the whole sediment. After passing through the screening wagons, the sewage runs through a brick conduit into a circular water chamber, furnished with horizontal paddles, where the iron, lime and sewage, are thoroughly mixed; thence the mixtures flows to the settling-tanks. the series of which is 266 ft. long by 73 ft. wide, built of brick in cement, with concrete bottoms. The bottom of each tank is an inclined plane, 3 ft. 6 in. lower at the entrance than at the exit

end, an arrangement that has been found fully adequate to retain the deposited sludge. After passing through the tanks the effluent water finally escapes over a weir, and so into the river. It is stated that the cost of the erection and maintenance of the works will be covered by a rate of 1¾d. in the pound.

The interest locally taken in this undertaking is very great, and the good example set by the Local Board of Buxton in visiting the sites of the various works suggested to them for imitation cannot be too well known. The plan may prove, however, to have much more than a local interest. Chalybeate water is of rare occurrence, and it is impossible that the exact conditions utilized by the skill of Dr. Thresh and Mr. Hague may be unique. But the attention that these works will cause to be given to the use of iron as a purifier has a wider scope.

THE SECOND LAW OF THERMODYNAMICS.

A VICE-PRESIDENTIAL ADDRESS BEFORE THE SECTION OF MECHANICAL SCIENCE AND ENGINEERING OF THE A. A. A. S., ANN ARBOR MEETING.

By J. BURKITT WEBB, Professor at Stevens Institute, Hoboken, N. J.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE second law of thermodynamics will be the subject of this address, and its object will be to make clear what that law is, and to show that Rankine's statement of it is the only correct one, and that it is entirely comprehensible and easily demonstrable. After calling your attention to some statements as to this law, and as to Rankine's way of putting it, no apology will be necessary for the choice of a subject, which ought already to have been fully and satisfactorily settled.

Professor Tait, quoting from Clerk-Maxwell, says: ". . . but when we come to Rankine's 'Second Law of Thermodynamics' we find that . . . its actual meaning is inscrutable.

"'The Second Law of Thermodynamics.—If the total actual heat of a homogeneous and uniformly hot substance be conceived to be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal.'

"We find it difficult, even in 1878, to attach any distinct meaning to the total actual heat of a body, and still more to conceive this heat divided into equal parts, and to study the action of each of these parts; but as if our powers of deglutition were not yet sufficiently strained, Rankine follows this up with another statement of the same law, in which we have to assert our intuitive belief that—

"'If the absolute temperature of any uniformly hot substance be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal.'

"The student who thinks that he can form any idea of the meaning of this sentence is quite capable of explaining, on thermodynamic principles, what Mr. Tennyson says of the great Duke—

'Whose eighty winters freeze with one rebuke
All great self-seekers trampling on the right.'" *

With all respect for the eminent physicist who gave this challenge, I would rather be responsible for these statements of the second law than for that with reference to his now celebrated "demon." "He will thus without expenditure of work raise the temperature of B and lower that of A, in contradiction to the second law of thermodynamics."†

Clausius gives as the "Second Fundamental Principle of the Mechanical Theory of Heat"—"Heat cannot of itself flow from a colder to a warmer body," and then applies it in thermodynamic investigations, and essentially

* "Miscellaneous Scientific Papers: with Memoir of Rankine," by P. G. Tait, M. A. London, 1881. Page 80 of Memoir.

† "Theory of Heat:" by J. Clerk-Maxwell, M. A. London, 1877. Page 328. (A and B stand for two portions of a uniformly hot and dense gas separated from each other by a thin wall.)

this statement, perhaps because of its inviting nature, is given in some of its many forms by numerous authors as the second law of thermodynamics; now it may be a law in the general theory of heat but not in thermodynamics, which treats of the relations between heat and mechanical energy.* This is a very different statement from Rankine's; it is simple and plausible—deceptively so—and amounts to a statement that the universe is running down, which is by no means axiomatic or easily demonstrable.

The formula for the maximum efficiency of a heat-engine is sometimes given as the second law of thermodynamics; but this is a consequence only of that law and of the form in which any continuously acting heat-engine must necessarily be built. Rankine calls this the "law of the efficiency of elementary thermodynamic engines," and the manner in which it results from his second law is simple and evident; it is, however, more frequently made to depend upon Clausius's fundamental principle, or its equivalent, by an argument based upon the fact that a perfect heat-engine is reversible, and a significant remark of Professor H. T. Eddy's is to be noted here. " . . . The necessity for the existence of the second law is to afford a basis for Carnot's principle, and any truth which affords such a basis is called the second law." † This is anything but a dignified rôle for a law of nature, which should never hold a position except in its own right, and the formulae of thermodynamics should not be made to depend upon truths chosen in this way.

A discussion of the various forms of the argument referred to would occupy too much space to form a part of this address; it may, however, be made the subject of a future paper. It is, however, to be remarked that the efficiency formula can be proved equally well on the supposition that heat will not flow of itself from a hot body to a cold one, or even that it will not flow in either direction,—we have only to reverse the pair of connected engines in the one case, or to run them in either direction in the other, to show the efficiency formula to be as correct as the hypothesis. It follows, therefore, that the truth of this formula is reflected in no degree upon the hypothesis of Clausius, and, further, this second fundamental principle, while it may be true, has nothing to do with the second law of thermodynamics, as given by Rankine, except in such way as all natural phenomena may be connected. We may add also that the necessity for this form of argument has greatly diminished, perhaps disappeared, since Carnot's time, the discovery of the nature of heat having furnished us with the means of deducing the second law directly from a consideration of its action.

The preference, too, should always be given to the proof which is direct, and which exhibits the mechanism of nature —which follows her straight path and reveals her steps. The *reductio ad absurdum* and other indirect arguments should only be used tentatively until the direct chain of evidence may be traced, and no scientific mind should be satisfied with a demonstration which would compel belief without removing apparent objections.

There is, however, much evidence, besides that furnished by Professor Tait, that Rankine is not understood; his statements are either copied verbatim, without explanation, or modified in a way to make this evident. But Rankine is not so much to blame; statements on well understood subjects are often in themselves quite unintelligible, and in new fields, words and phrases are not always at hand to completely express new ideas. Language is meant to excite and direct the imagination, and rarely does a form of words, when first heard, convey a definite and correct meaning unless the imagination meets it with pictures of all possible ways in which it may apply. Sometimes even it is necessary to understand the subject before statements in regard to it can be appreciated, and I confess that until I had worked up this subject myself Rankine's statements were troublesome; now, however, they seem so reasonable that I shall endeavor to lead you to the same opinion and to convince you that of the three statements to which reference has been made, only Rankine's is indeed the second law of thermodynamics.

Let us inquire first what we should

* See Rankine's "Steam Engine." Pages 223 and 299.

† "Thermodynamics," by H. T. Eddy, C. E., Ph. D. New York, 1879. Page 18.

expect for the second law after the first law has stated that heat and work are mutually convertible and convertible in a fixed ratio. It is manifestly appropriate that the second law state the agency by which such a conversion may be effected, and also the rate at which it will proceed.

To my classes in this subject I have for two or three years past given the following statement of the second law:—

A quantity of heat, W′, *may be employed as an instrument for the conversion of another quantity of heat,* W, *into work, or for the conversion of a quantity of work,* A, *into heat, and the converted quantity will be proportional to the convertor quantity for a given change of volume or of entropy.*

To realize the meaning of this statement, let us imagine the simplest physical air-engine;—we need no fly-wheel, valves, etc., but simply a vertical cylinder of infinite height and unit section with non-conducting walls; the cylinder bottom must be permeable to heat and the non-conducting piston must be loaded with a pressure varying so as to be always but a differential less than the gaseous pressure beneath it. If we fix a finite limit to the total amount of heat which the engine shall be capable of transforming into work, then the height of our cylinder need not be infinite.

But this is no more than the shell of the engine;—we will suppose the piston to be at such a point that the cylinder shall have a volume of one cubic unit, and we will now put in it, say one unit of mass of the molecules of a perfect gas;—the molecules may be supposed to be lying on the bottom, like dust, or they may be equally distributed through the space, as in any gas, but they will be *devoid of motion.* We have now added the muscles, perfect in all respects, except that they are dead, and our engine is no more capable of performing its functions than would be a dead crab or lobster. The engine is not capable of transforming heat into work, for the reason that *the agent* by which such a transformation may be effected *is not present;* the space exists through which the piston may move, but, inasmuch as the molecules exert no pressure against the piston, there can be no question of work. Work is the product of force into space, and one factor, the characteristic one, is here wanting.

To obtain the needed pressure we must heat the molecules of the gas to the absolute temperature τ, *i. e.*, we must store in the molecules an amount of kinetic energy proportional to τ, and calculation shows that the pressure per square unit, which will then result from the rebounding of the molecules from the piston and walls of the cylinder will be proportional to this amount. It will also, should the volume be increased by changing the position of the piston be inversely proportional to that volume. In the ordinary formula which connects the volume, pressure and temperature of a perfect gas,

$$pv = \mathrm{R}\tau,$$

we have but to suitably change the value of R, say to R′, to be able to write

$$pv = \mathrm{R}'\mathrm{W}',$$

where p and v are the same pressure and volume as before, R′ a new constant and W′ the convertor quantity of energy, referred to in the law as given above, and which we have stored up in the mass of the molecules to act as the agent or instrument for the conversion of heat into work or *vice versa.*

I wish to emphasize this point:—the engine consists of mass and energy, and this mass is divided into two portions—the metallic parts of the engine, which are made in the machine shop, and the molecular masses, which we will suppose to be supplied in the physical laboratory. The animal is not perfect without life, it is not an animal and cannot perform the functions of one, and so it is this stored-up energy, which is the real agent in the engine, and without which the engine may be said not to exist.*

Let us now see what is Rankine's last and most general statement, in which he generalizes the second law. "The effect of the presence in a substance of a quantity of actual energy, in causing transformation of energy, is the sum of the effects of all its parts." Here he distinctly places energy before us as the agent for the transformation of energy, and I believe that *nothing but energy can act as the agent,* and that the gener-

* Rankine states somewhere that it is the working fluid which is the fundamental part of the engine, but I think that in this Rankine falls behind himself.

al law underlying all such transformations may be stated, perhaps, in the following form:

Every conversion of energy from a form A *into a form* B *can be effected only through the agency of a quantity of energy and* (I venture to add) *this agent, or convertor quantity, must possess at once the characteristics of* A *and* B.

In the air-engine the agent has temperature and pressure, respectively, characteristics of heat and work; in the dynamo the field is characterized by both electrical and mechanical tension. In fact, this seems to me to be the only way in which we can logically conceive of such a transformation, and the only way in which observed transformations occur.

Now, our agent quantity of thermal energy must act without expense of energy to itself, for this is the peculiarity of agents, that they always make themselves safe when they do business for others; therefore the expansion must be isothermal, and a source of heat at the temperature $\tau + d\tau$ must be applied to the conducting bottom, so that the gas will be caused to expand with no loss of its own energy. In reality, *there is no other expansion than isothermal for the conversion of heat into work;* as to adiabatic expansion, it is never used for that purpose, nor can it be; it furnishes, simply, a means of lowering the temperature of the gas if the engine is made to run in a cycle, and the temperature may be lowered equally well by regenerator plates, the only difference being in the form in which the energy taken away from the agent is stored up. Adiabatic expansion does not take heat from the source and transform it into work, and the only expansion that does is isothermal; and in any mixed expansion it is only the isothermal element that is of any value for this purpose.

Now, the gas, in expanding from the volume unity to the volume v, converts a quantity of heat into work, and this quantity will be infinite when v becomes infinite. Also for any given change of volume, say from v to v', the amount of heat changed into work will be proportional to the pressure, that is, proportional *to the agent energy;* but this is just what the second law states, and the proof is therefore seen to be of the utmost simplicity, depending simply on the mutual proportionality of work done, the pressure, the temperature, and the agent energy, or total actual heat (as Rankine calls it) of the working substance.

Let us look now at the behavior of the molecules; as each rebounds from the hot bottom its stock of agent energy receives into itself a portion of the heat W, which is to be changed into work. The molecule is driven away with increased velocity by the hotter, or more energetically vibrating, molecules of the bottom; it carries its added energy to the piston, and gives only this quantity up in the form of work. The reason that it gives it up is simply that, if the piston be moving up with the velocity V, the molecule will rebound with a velocity reduced by 2V. As the volume increases the pressure falls, on account of the diminished frequency of the rebounds per unit of surface, and therefore the conversion of heat into work must proceed more slowly, but for any and all particular changes of volume the speed of conversion may be increased to any degree by means of a proportional increase in the amount of the agent. It is interesting, also, to notice the thing in another light; the energy has to be carried over the distance from the hot bottom to the moving piston, which distance increases directly with the increase of volume, and the time required to carry a certain amount will be increased as this distance increases; the way to secure a more rapid conveyance throughout is to increase the speed of the carrying molecules, *i.e.*, the amount of agent energy. Note also that the amount carried per molecule will be at the same time increased.

The molecules have been spoken of as moving directly toward and away from the piston, whereas they should move in all possible directions; it is well known, however, that this involves no error in the result, and scarcely one in the conception.

Let us see now whether there are any real difficulties in Rankine's statements. To understand them most easily, commence with his "General Law for the Transformation of Energy," already quoted, and proceed backward; we come first, then, to a graphical representation of the second law. After explaining the quantities in his diagram and

stating the known relations between them, he requires us to suppose the temperature τ, to be divided into n equal parts. Now, we can divide nothing except quantity into equal parts, and therefore Rankine regards temperature as a quantity, and so it is. We have only to remember that τ is the temperature of the agent, and therefore τ is the amount of agent energy in terms of a suitable unit; in fact, the statement that a unit mass of gas is at a temperature τ is equivalent to saying that it possesses τ units of thermal energy, the unit used being the energy required to raise this amount of gas one degree in temperature.

It is the agent energy, then, which we are to divide by n, and we are afterward told that the parts will be "similar, and similarly circumstanced." Let us suppose a molecule heated from absolute zero to the temperature τ, by adding n equal increments of energy, these increments can differ only in the order in which they may be added, and once added this distinction vanishes, and they become merged into the quantity of energy contained by the molecule.

On the scale of the thermometer the degrees all have their places, and the last degree added is necessarily the first one subtracted; but no similar conception can be applied to the energy of the molecule, and we can imagine no connection between the last increment added and the first subtracted, or any difference whatever between the increments, whose sum is the total energy of the molecule.

"Similar and similarly circumstanced" mean, therefore, simply devoid of absolute and relative differences.

In this graphical treatment Rankine shows isothermal expansion, and his second law applies essentially to this expansion.

Rankine's next statement (proceeding backward) of his law is the second one criticised by Maxwell, and it supposes nothing more than the division of temperature already discussed. In some earlier editions the first formula under the statement is in error by the omission of τ; it should read

$$\tau\frac{d}{d\tau}=\mathrm{Q}\,\frac{d}{d\mathrm{Q}},$$

as it stands in the present edition.

We next come to a seemingly more general and comprehensible statement of the law which speaks of "the total actual heat." Now, Rankine has defined this, and it is simply the kinetic energy of the molecule, or that portion of the heat furnished to cause a rise of temperature from absolute zero to τ, which remains in the body as heat.

Inasmuch as Rankine's first statement of his law seems the more general, insomuch has it led, as I believe, to a false comprehension of its meaning. It may seem more general in this way: In the

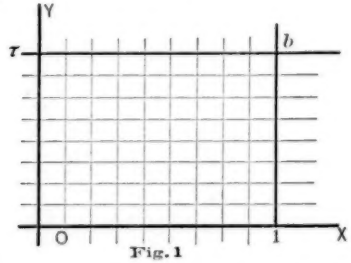

Fig. 1

figure let x represent mass, and O1 a unit of mass; also let y represent temperature; then, if a suitable unit of temperature has been chosen, the area O1$b\tau$ will represent the heat energy contained in the unit mass at the temperature τ. Now, we may cut this energy into n equal parts by means of equidistant vertical lines, and these lines will cut the mass also into n equal parts; in this case it requires no scientific imagination to see that the effect of the heat belonging to any one of the increments of mass must be the same as that belonging to any other, and that the heat of one increment is similar to that of another, though not quite similarly circumstanced, because the increments of mass have different positions in space.

Such a method of dividing the total actual heat is altogether too simple for the use to be made of it, and no such statement can pass for a law of thermodynamics; it is simply the law of homogeneousness. If the mass of a homogeneous body be divided into n equal parts, then the effects of these parts are the same in making up the total weight, value, density, etc., etc., of the body.

Rankine leads up to his statement in an unfortunate way, perhaps, in empha-

sizing the fact that every particle is equally hot without telling why; of course every particle must be so, or we could not speak of the body as having a temperature τ, and the right line τb in the figure would disappear; this would interfere with the argument, which depends essentially upon our dividing a temperature τ into equal parts.

Rankine intends no such division; he says, in fact, "Let unity of weight," and we may take a differential unit, and so put such a division out of the question. The division intended was by horizontal lines, and this makes the first statement of the law identical with the second, only in the one he says "total actual heat," and in the other "temperature." He commences by saying "heat," because heat is energy and is the real thing he refers to, and he changes to "temperature," because that is the practical way of measuring the amount of this energy, being proportional to it.

I believe, then, that this is the one and only second law, and it follows that because our agent is a quantity of energy, which resides only in mass, and because different substances do not differ in mass —that being a common or fundamental property of all substances—therefore, the particular working substance used has no effect upon the result.

To further illustrate the division into equal parts, which Rankine intends, let us consider the pressure produced by a mass under the action of gravity.

The rectangle O1$b\tau$ will represent this pressure for a unit of mass if we lay off the acceleration of gravity on OY so that $\tau = g$. We shall have the same two possible divisions as before; that by vertical lines will correspond with the fact that each equal element of mass contributes its own equal share of the pressure, while for the horizontal division we must regard the force of gravity as made up of equal increments, each of which takes equal part in producing it. Now, it is a matter of no consequence whether we say "divide the acceleration g into n equal parts" or, "divide the force gm;" in the one case each increment will be represented by a portion of the line OY and will correspond to an increment of temperature, in the other each will be represented by a horizontal band of the area O1$b\tau$ and will correspond to an increment of total actual heat. There may be more difficulty in conceiving of a division of the acceleration or of the temperature into equal parts, than of a division of the energy or of the force, or weight, but this difficulty lies in conceiving of either acceleration or temperature as a *quantity* and not in the division of that quantity; both, however, must so be conceived before any mathematical or other exact treatment of them can be made. If we conceive of a force or weight, P, as applied at a particular point (let it be attached, say, to the geometrical line rq) without our knowing the acceleration of gravity or the mass, but only their product P, then, while both divisions of P into equal parts are still possible, that into horizontal bands would seem to be the most natural; any variation in P would usually, I think, be considered as resulting from a change in the acceleration, in which change each equal part will have the same effect in producing the pressure P, and they will all be similar and similarly circumstanced.

Fig. 2

If the camel's back does break, it is the first hair as much as the last that does it —this illustration, however, corresponds to the vertical sub-division, and we must suppose the fully-loaded camel to move through space in such a direction that gravity shall continually increase, in which case it will be the first increment of g as much as the last, which will tend to break his back.

Let us look again at the formula for efficiency, which flows directly from Rankine's law in a simple and evident manner. Our infinite-cylinder engine needs no such formula, for it works *at* a temperature τ and not between two temperatures τ and τ'; it transforms, also, all the heat into work. Mechanical considerations, however, require us to forego infinite cylinders and to build engines

that run in cycles, and for these this formula is needed. Every engine running in a cycle is a double engine and consists of an engine proper and a compressor. When the piston has risen as far as the practical height of the cylinder admits, during which rise it has converted all the heat used into work, some method must be taken to repeat the process with the same engine. The engine is therefore used as a compressor for re-compressing the gas, during which operation all the work done on the gas is transformed into heat, and we have only to cool the gas, by adiabatic expansion or a regenerator, before compression in order to get our gas back again to its original density and have left a margin of heat permanently transformed into work, in accordance with the efficiency formula.

It should be remarked that no special reference has been made in this address to anything but a perfect-gas engine because the theory of the action of such an ideal gas should be perfectly clear before the more complicated action of real substances, especially liquids and solids, is considered; Rankine's law, however, covers all such cases, and his definitions and formulæ are framed to include them.

Many other points have necessarily been left untouched, but if I have made clearer how heat and, therefore temperature, may be supposed to consist of a number of equal parts, and how these parts cannot differ in their situation or action, and convinced you that Rankine's is the real and only second law of thermodynamics, my object will have been accomplished.

Note.—A mathematical demonstration of the relation between the total actual heat of a perfect gas and its pressure upon the walls of the containing vessel will be discussed in a future number of this journal.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—The transactions of the Society for July and August contain valuable reports upon preservation of timber.

The July issue contains the report of a committee presented at the last convention. The August number contains twenty appendices to the report.

A paper on the Preservation of Forests, by Francis Collingwood, with discussions on both of the above topics, make up the September number.

The Engineers' Club of Philadelphia.—The latest issue of the Proceedings contains:

Wrought Iron Stringers for the Substructure of Railroads. By William Lorenz.

The Pennsylvania Schuylkill Valley Railroad Tunnel at Phœnixville. By M. Van Harlingen.

Sources of Pollution in Storage Reservoirs. By A. Harvey Tyson. Discussion.

Strengthening the West Main Abutment of Chestnut Street Bridge. By J. Milton Titlow. Discussion.

The Construction of Street Railways in the United States. By William Wharton, Jr.

Pertaining to Isthmian Transits. By Preston C. F. West.

The Water Works of Columbus, Ga. By Jacob H. Yocum.

ENGINEERING NOTES.

One of the many odd callings which the production and pipe line transportation of petroleum have made necessary is one known as "chasing the scraper." Crude petroleum is run from the oil regions to the refineries at the sea board, a distance of 300 miles, in iron pipes. It is forced over the high hills that intervene by powerful pumps. Much of the way it runs by its own gravity. The pipes are constantly becoming clogged by sediment and paraffine. To clean them out an iron stem, 2½ feet long, to which are attached circular steel scrapers, fitting loosely in the pipes, is placed in the pipe at regular periods. This is forced along the line by the pressure of the oil behind it. It is necessary to keep track of this scraper, in order that its exact location may be constantly known, so that if it is stopped by any obstacle it may be readily discovered, and the obstacle removed. The noise made by the scraper against the iron pipes as it moves along their interior would not be heard by an untrained ear, but certain servants of the Pipe Line Company are able to follow it on its journey by the noise, and never lose its situation. These men are the scraper chasers. They are stationed in relays three or four miles apart along the line. One chaser will follow up and down mountains, across ravines and through streams and swamps until he reaches the end of his section, when another man takes up the chase and follows it until relief is reached, and so on until the course of the scraper is run. The work is one of hardship and danger, owing to the character of the country through which miles of the pipe line is laid. If a chaser by any mishap is thrown off the track of the scraper, and it becomes clogged before he can recover its position in the pipe, the cutting of the pipe for long distances is frequently made necessary that the missing object may be found, a work that is accompanied by much expense and labor.

The Forth Bridge.—The Forth bridge will connect Queensferry on the Fife shore of the estuary with South Queensferry on the Linlithgow shore, the total length of the bridge

being 2,700 yards, or 1,900 yards between high-water mark on the two sides. Beginning at the Queensferry side, the bridge is carried out 1,780 ft. on a series of nine piers of solid masonry, faced with cyclopean blocks of Aberdeen gray granite Each of these piers will be 130 ft. in height above high water and the span between them is 160 ft. The fourteenth pier from the shore is called the cantilever tower, as upon it will rest not only the end of the girders, but the end of the shoreward arm of the south cantilever. This tower has, in consequence, been made of great size—the basement or cut-water being 103 ft. in length and 52 ft. in thickness. The crucial part of the scheme is that which carries the railway over the deep water of the Forth—the free navigation of the channel for the largest ships having to be conserved. From three sets of granite piers, arranged in groups of four columns each—the central set at Inchgarvie, and the others about 1,800 ft. on each side—a superstructure of steel will be reared supporting bracket-like arms, two of which, with a separate girder in the center, will form an apparent arch with a span of 1,700 ft., or about one-third of a mile, and with a clear height above high water level of 150 ft. There will be two such spans and two half-spans of 680 ft. each thrown shoreward to cantilever towers on either shore. Foundations for three sets of cantilever piers were obtained by sinking iron and steel caissons, 70 ft. in diameter, to the solid rock at North Queensferry and Inchgarvie, and to the boulder clay at South Queensferry. These caissons were filled with concrete to low-water mark, and above that the piers have been continued to a height of 18 ft. above high water in solid masonry, faced with rough-hewn blocks of granite. These cylindrical masses of concrete and masonry from the base of 70 ft. gradually diminish in diameter to 50 ft. In the Inchgarvie pier, the central points of its quadruple members form an oblong, 270 ft. from north to south, and 120 ft. from east to west. In each of the other two great piers the oblong is only 155 ft. from north to south—the distance of 120 ft. from east to west being preserved. On each of these masses of stonework the steel superstructure forming the cantilever bridge will be erected, the three cantilevers being precisely the same in character. On the top of each of the four stone columns forming the pier is placed a skewback, constructed on the cellular principle of thick steel plates. Each skewback is firmly bolted to the masonry by means of a bed plate and tying-down bolts, carried deep down into the masonry of the column. Lengthwise these skewbacks are united by cylinders 12 ft. in diameter, while crosswise the binding is by lattice girders. From the center of each skewback rises a cylindrical column 12 ft. in diameter to a height of 340 ft., each member bending inwards. Viewed collectively along the length of the bridge, the four look like two great triangles with flattened tops. Their basal distance is 120 ft., center to center, but at 340 ft. up they have converged to 33 ft. On the inner side of each of these columns cylindrical shafts of the same type are carried from the foot of the one column to the top of the other, and form an X bracing; while atop the four main columns and the cross-bracing are knit together in a rigid framework by lattice girders of a maximum depth of 12 ft. From the outer ends of the skewbacks are thrown brackets, also cylindrical in shape. Each is 680 ft. in length, while from the top of the 340 ft. columns girders descend to within 40 ft. of the end of the brackets or arms. The cantilever bracket and sloping girder are firmly braced together by tubes and girders of diminishing sections, as the end of the arm is neared. A main support to the sloping girder is a great cylindrical shaft, which also rises from the skewback on the outer side of each of the main vertical columns. The two bracket arms connected with each pier being of the same dimensions in all respects, exactly balance each other. The various thrusts and strains—vertical, horizontal, and diagonal—involved in their own weight and the load they have to carry, all meet in the connected skewbacks, where they counterpoise each other. The cylindrical shape for the brackets and upright columns has been chosen as best fitted to resist compression, while the girders are used where it was necessary to provide for a tension strain. At the present time the number of men employed at the bridge is close on 2,500. At the South Queensferry end, out to 2,000 feet from the shore, the whole of the granite piers for carrying the girders have been founded and reared, with rounded cutwater, to a height of 18 ft. above high water. Each pier constitutes an oblong, placed east and west, with a base at low-water level of 52 ft., and with a width of 23 ft. The extreme or northernmost pier of this series is the cantilever tower already referred to, and it, too, is up to the same height above high-water level. The steel girders which will form the crown of this part of the structure are being constructed on the top of the stone caps of the piers at the level just mentioned. Strong staging with supports rising from the bottom of the shallow water is reared between each pier; on this the girders are put together, and, as each span is completed, the girder is dropped on the pier, and the staging removed farther out. The girders will be simultaneously raised to their permanent position at the level of 130 ft. above high water by means of hydraulic jacks, thereby avoiding the expense and inconvenience of high staging. When the jacks are in position, hydraulic power will be applied, and the whole of the girders, forming a viaduct nearly 600 yards long and weighing close on 2,000 tons, will be raised about a foot at a time. As it rises, the ironwork will be shored up and the masonry of all the piers built under it. From the new platform thus formed, another lift will be made, and the operation will be repeated until the necessary height is attained. On the north side the corresponding five piers of the viaduct—all, however, founded on dry land—are carried up to a minimum height of 38 ft. above high water; the girders put together upon them have been finished, and preparations are completed for lifting them into position in the same way. Of the south main pier, for carrying the large cantilevers, three of its group of four members have been founded and two reared to their height of 18 ft. above

high-water level, the third above water level. This is the full height of all the columns of the three great piers. At the fourth, the north-west member of this group, the caisson tilted over and was crushed. A timber casing is being constructed around it, which will practically be a coffer-dam; and when this is completed, the caisson will be pumped dry and the plates renewed. On Inchgarvie the works connected with the great central pier are also well advanced. At one time the island was considered a desirable resting-place for the central supports of the bridge, but it is curious to note that none of the four members of the central pier are actually on *terra firma*. The line of the bridge did not admit of that being done. Of this central pier, two columns are just beyond the west end of the island, and the other two 270 ft. further south. The former two columns are up to their full height above water; for the south-east member the caisson is now being sunk, while the caisson of the south-west member has just been floated into position. The depth of each caisson varies according to the distance it has to be sunk into the estuary bed. The deepest yet sunk in connection with the bridge is 71 ft. below low water. But all are filled solid with concrete and carried up to low-water level. Above this the column is built up of solid rubble masonry from Arbroath, faced with blocks of granite. At intervals iron belts are inserted in the masonry. On the Fife shore the four members of the main pier are all founded and finished. Two are on the land, and the other two are in the water, here of considerable depth. On this pier a portion of the permanent cylindrical tubing is stretched between its western members; and here, too, may be seen in position the bed plate on which the skewback will rest, with its holding-down bolts. On each column there are no fewer than 58 of such bolts each a bar of steel 2½ in. in thickness, with a screw at the top 3 in. in diameter, secured by a nut. The bed-plates are 3 in. to 4 in. in thickness. The bottom of the skewback will be of similar construction. On three out of the four columns of each pier, the holes in the bottom of the skewback, through which the bolts will pass, will be cut of an oblong pattern, 9 in. or so in length by 4 in. across, so as to allow for a certain amount of movement of the cantilevers on the piers. On one column of every pier the bolt holes in the skewback will be circular, and just wide enough to admit the bolt, so that too much play in one direction may be prevented. The piers for carrying the girders inland to the ridge of the high ground between North Queensferry and Inverkeithing Bay are completed to a certain height, and everything is in readiness to lift the viaduct girders which are resting upon them. In the bridge workshops at Queensferry has sprung up an immense engineering establishment, where the whole operations connected with the rolling, bending, planing, and drilling of the steel work of the superstructure of the bridge are being proceeded with. Most of the machinery in the works had to be specially invented or adapted to meet requirements. Almost every machine in the place has some ingenious feature for saving labor, of which Mr. Arrol, one of the contractors, is the author. Among other processes going on is the drilling of rivet holes by multiple machines, working simultaneously a large number of spindles. In all, something like 150 spindles are at work mostly night and day, and from first to last something like 5,000,000 of rivet holes have to be drilled. 45,000 tons of steel will be used in the construction of the bridge, exclusive of 3,000 tons in the viaduct girders, and that of the tubes for the cantilevers about 5½ miles, have to be made. Of the steel work nearly 6,000 tons are ready for erection. The total amount of building done up to the present time is:—Granite set, 220,000 cubic feet; concrete, 43,000 yards; rubble masonry, 25,000 yards. The engineers for the bridge are Mr. John Fowler (engineer-in-chief) and Mr. B. Baker; the contractors, Sir Thomas Tancred, Bart., and Messrs. W. Arrol, Falkiner, and Phillips. The chief of the engineers' staff at Westminster is Mr. A. D. Stewart, and at Queensferry Mr. P. W. Meik, with five or six assistants and four outdoor inspectors; while the contractors are represented at the works by Messrs. R. E. Middleton, W. Westhofen, A. Biggart, and others.

IRON AND STEEL NOTES.

M. P. Gabriel gives the following method of tempering steel, in the *Revue Chronometrique*:—Cyanide of potassium is dissolved and red heated in a metallic or earthen crucible; the pieces of steel are then immersed in the liquid until red, and afterward plunged in water. This process is said to give great satisfaction, and many advantages are claimed for it. The temper is said to be harder, and if a finished piece is under treatment the polish is not lost. It will show a grayish tint, but the original polish will reappear immediately, if a piece of polished wood with the finest rouge is passed over it. It is also said that if the steel has been well annealed, and not put out of shape by the file or the hammer, it will come from the crucible perfectly straight; arbors 4 or 5 centimeters long are not deformed, if tempered by this method. It is recommended as particularly advantageous for tempering escapement springs.

Some curious statements on tempering steel are made in a paper published in *Dingler's Polytechnic Journal*, vol. 225, by Herr A. Jarolimek, "On the Influence of the Annealing Temperature upon the Strength and the Constitution of Steel" Hitherto it has been generally considered that to obtain a specified degree of softness it is necessary to heat the hard steel to a particular annealing color—that is to say, to a definite temperature—and then allow it to rapidly cool. Thus, for example, that steel might anneal—be tempered—yellow, it had to be heated to 540 deg., and the supposition was formed and acted upon that it must be allowed only a momentary subjection to this temperature. Herr Jarolimek says the requisite temper which is obtained by momentarily raising the temperature to a particular degree, can also be acquired by subjecting the steel for a longer

time to a much lower temperature. For example, the temper which the annealing color—yellow—indicates can be obtained by exposing the hard steel for ten hours to 260 deg. of heat; in other words, by placing it in water rather above the boiling point.

A PAPER was read recently before the Royal Society "On Magnetization of Iron," by Dr. Hopkinson. It contained an account of the results of experiments which have been made on a considerable number of samples of iron and steel of known composition, including samples of cast iron, malleable cast iron, wrought iron, ordinary steels, manganese, chromium, tungsten, and silicon steels. The electrical resistance and the magnetic properties are determined in absolute measure. Amongst the electrical resistances the most noteworthy fact is the very high resistance of cast iron—as much as ten times that of wrought iron. The fact that manganese steel is almost non-magnetic is verified, and its actual permeability measured. The action of manganese appears to be to reduce the maximum magnetization of steel, and in a still greater ratio the residual magnetism, but not to affect the coercive force materially. It is shown that the observed permeability of manganese steel containing 12 per cent. of manganese would be accounted for by assuming that this material consists of a perfectly non-magnetic material, in which are scattered about one-tenth part of isolated particles of pure iron. Some practical applications of the results are discussed.

RAILWAY NOTES.

RAILROADS IN CENTRAL ASIA.—The Central Asiatic Railway will now be pushed forward very energetically, in pursuance of the recent imperial order. Gen. Annenkoff, charged with the direction of the necessary works, has already started for the Trans-Caspian, and will be there before you receive this letter. Great attention is paid in Russian circles to this railway, which will be of the utmost importance, not only strategically, but also in its relation to commerce. The line will go in a direct southeastern direction to Kakhka, which place is about half-way between Askabad and Sarakhs, but then the road will take a northeastern direction to Merv and further to Bourdalik, on Amu-Daria. In this way the railway, which has hitherto closely followed the Persian frontier, will suddenly branch off at an angle and be continued to a distance of about 50 miles from the new Russo-Afghan boundary, which is thought will be settled by the negotiations between England and Russia. The considerations which have determined the government to choose this direction for the railway are commercial ones, and the line now will be connected with the principal fluvial communications in the Russian possessions in Central Asia, also with the great caravan roads. The expenses of keeping up the railway will be serious. They are calculated at nearly 2,000,000 roubles a year, and during the first year or two the receipts will scarcely amount to 200,000. After that time, when the Central Asiatic merchants have learned to appreciate the new means of communication, the traffic may be expected to largely increase. It is said on good authority that the Central Asiatic Railway will be continued next year across the Bokharian territory to Samarkand, and from thence to Tashkend, the residence of the Turkestan Governor-General. The distance between the two towns is 260 English miles.—*St. Petersburg Letter to the London Daily News.*

RAILWAYS OF THE UNITED STATES.—The total number of miles of railroad in the United States at the close of 1884, was 125,379, of which 3,977 miles were constructed during the year—the rate of increase being 3.17 per cent. The number of miles making returns of their share capital and funded and floating debts equalled 125,152, against 120,552 for 1883, the increase being 4,598, the rate of increase being 3.8 per cent.

The share capital of the mileage in operation in 1884 equaled $3,762,616,686, against $3,708,060,583 in 1883, the increase equaling $54,556,103, the rate of increase being about 1.4 per cent.

The funded debts of all the lines at the close of the year aggregated $3,669,115,772, a sum $168,235,858 in excess of the total of 1883 ($3,500,879,914), an increase of nearly 5 per cent.

The other forms of indebtedness of the several companies at the close of the year equaled $244,666,596, against $268,925,285 for 1883, the decrease being $24,258,689. The total share capital and indebtedness of all kinds of all the roads making returns equaled at the close of the year $7,676,399,054, a net increase in the year of $198,533,272 over the total of 1884 ($7,477,865,782), the rate of increase for the year being about 2.6 per cent.

The cost per mile of all the roads making return as measured by the amount of their stocks and indebtedness equaled very nearly $61,400, against $61,800 for 1883.

The gross earnings or receipts of all the lines from which returns were received for the year equaled $770,684,908, of which $206,790,701 were received from transportation of passengers; $502,869,910 from transportation of freight; $7,464,099 by lines the returns of which were so incomplete as to preclude their use in the tables giving the general results—the sources of income, amount of tonnage moved, etc., etc.; and $53,749,997 from the transportation of mails and express matter, from investments, and from the sales of lands applicable to the payment of interest or dividends.

The number of persons transported in 1884 by all the lines was 334,814,529, against 312,686,641 for 1883, the increase for the year being 22,127,888, the rate of increase equaling 7.8 per cent.

The number of passengers carried one mile in 1884 equaled 8,778,581,061, against 8,641,309,674 for 1883, the increase equaling 237,271,387 persons carried one mile, the rate of increase equaling very nearly 3 per cent.

The distance traveled by each passenger in 1884 equaled 26.24 miles; in 1883, 27.32 miles.

The amount received per passenger per mile

equaled 2.356 cents in 1884, against 2.422 cents in 1883. Had the passenger rates for 1883 been maintained for 1884, the earnings from this source would have equaled $212,617,233, a sum $5,826,532 greater than that received.

The number of tons of freight transported on our railroads in 1884 equaled 390,074,749, against 400,453,439 tons in 1883, the falling off equaling 10,378,690 tons, the rate of decrease being about 2½ per cent. The value of the tonnage moved in 1884, estimating its value at $25 the ton, equaled $9,751,868,725.—*Poor's Manual.*

ORDNANCE AND NAVAL.

VENTILATING SHIPS.—We have just received from Mr. J. M. J. Barton particulars of his system of ventilating ships by means of a close furnace placed in the upper part of the vessel. A series of pipes extends from the several compartments in the vessels to the furnace, which is closed at the bottom, and the doors are made to fit very closely, so that no air can pass to the fire except through the pipes provided for that purpose. The fire in the furnace causes a draught, and as no air can enter except through the pipes, a powerful suction will be produced, and the foul air in the several parts of the ship will be drawn into the furnace. Fresh air will naturally pass into the compartments through passages provided for the purpose. The inner ends of the pipes are closed by gratings, to prevent the entrance of live coals. This device can be applied in any marine vessel, but is especially adapted for steamers, as the furnace of the boilers could be utilized. In sailing vessels a special furnace would have to be provided.

THE steel armor-plated barbette ship Rodney, ten guns, 9,600 tons, 7,000 horse-power, returned to Chatham Dockyard after a successful series of trials of her engines. The official trial was of the most satisfactory character. With a natural draught the following results were obtained:—Mean indicated horse-power, starboard, 4,222; port, 4,040; collective, 8,262; steam in the boilers, 89 lbs.; vacuum in condensers, starboard 28.5 in.; port, 28 in.; revolutions per minute, starboard, 94; port, 93; mean pressure in cylinders, starboard, high, 45.61; low, 11.74; port, high, 43.44; low, 11.50. With forced draught and enclosed stokeholes, the following results were obtained:—Mean indicated horse-power, starboard, 5,598.55; port, 5,558 21; collectively, 11,156.76; steam in the boilers, 90 lbs.; vacuum in condensers, starboard, 27.5; port, 28; revolutions, starboard, 104; port, 103; mean pressure in cylinders, starboard, high, 59.75; low, 12.83; port, high, 60.10; low, 12.78. The rate of speed attained was beyond that anticipated, over 17 knots per hour being made, notwithstanding the fact that the vessel's bottom was foul through having been in the basin at Chatham so long. The machinery worked with smoothness and regularity, the boilers generating an ample supply of steam, and no hitch occurred.

AT the meeting of the Royal Geographical Society held recently, Sir Peter Lumsden read a paper on the countries and tribes he has recently visited west of Afghanistan. He gave an interesting description of the geography of the Murghab valley and the customs of its people, and quoted a singular account of the Numaksar, or salt lakes of Yar-oilan, visited and described by Captain Yate. He said:—"The valley of the lake from which the Tekke Turkomans from Merve get their salt is some six miles square, and is surrounded on all sides by a steep, almost precipitous, descent impassable for baggage animals, so far as I am aware, except by the Merve road, in the northeast corner. The level of the lake I made to be about 1,430 ft. above sea level, which gives it a descent of some 490 ft. from the level of the connecting ridge, and of some 950 ft. below the general plateau above. The lake itself lies in the center of the basin above described, and the supply of salt in it is apparently unlimited. The bed of the lake is one solid mass of hard salt, perfectly level, and covered by only 1 in. or 2 in. of water. To ride over it was like riding over ice or cement; the bottom was covered with a slight sediment, but when that was scraped away the pure white salt shone out below. How deep this deposit may be it is impossible to say, for no one has yet got to the bottom of it. To the east of the dividing ridge is the second lake, from which the Saryks of Penj-deh take their salt. The valley in which this lake is situated is much the larger of the two. The valley proper is itself some fifteen miles in length by about ten miles in breadth. The salt in this lake is not smooth as in the other, and did not look so pure. It is dug out in flakes or strata, generally of some 4 in. in thickness, is loaded into bags and carried off on camels for sale without further preparation."

THE LOADING OF HEAVY ORDNANCE.—The muzzle of a 9-inch gun was blown off at Shoeburyness during some experiments with a Hadfield steel shell a few weeks since, and the fragments have been received at Woolwich for examination in the Royal Gun Factories. So little damage has the gun suffered that it could still be fired effectively, and the extra weight of the projectile, coupled with the strength of the powder charge, amply account, in the opinion of the authorities, for the accident having occurred. The event has, however, says the *Morning Post*, brought into prominence the fears which have been felt for some time past, that the advocates of extreme charges are sailing dangerously close to the wind. The artillerists by whom these charges are fixed are satisfied by repeated experiments of the enduring qualities of the new guns which are now being manufactured, but it is worthy of remark that the cartridge of the 10.4 gun is to be 190 lbs. of powder, whereas the much larger gun of 15.5 inches (the 38-ton gun) has a charge of 160 only. It should be borne in mind, however, that the violent and refractory qualities of gunpowder have been of late subdued beyond all expectation. When the "rifle large grain" was the only powder used for great guns, pressures of 60 tons were by no means uncommon, but

as soon as the pressures began to be measured, they began to diminish until the introduction of the pebble powders, scientifically compounded, sized and shaped, brought about the rule of 25 tons as the maximum pressure. Since that, however, the cocoa and prismatic powders have been invented, and the present "brown" powder, which is adopted for all the large guns in the service, is found to give the full velocities with pressures seldom exceeding 16 tons. In theory, therefore, the gunners might safely fire charges of the new gunpowders three, if not four times the weight of those which were fired when the subject of explosives was less understood.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

PAPERS of the Institution of Civil Engineers:

No. 2046.—The River Buffalo; its Flow, etc. By William Bloomfield Tripp, M. Inst. C. E.

No. 2049.—The Cape Government Railways. By William George Brounger, M. Inst. C. E.

No. 2059.—The Electrical Regulation of Steam Engines. By Peter William Williams.

No. 2071.—Public Works of the Orange Free State. By Gustave Hallé, Assoc. M. Inst. C. E.

No. 2077.—Purification of Water by Means of Iron. By William Anderson, M. Inst. C E., and George Henry Ogston, Assoc. M. Inst. C. E.

No. 2078.—Pollution of the Thames near London. By Robert William Peregrine Birch, M. Inst. C. E.

Introduction to Poor's Manual of the Railroads of the United States for 1885. New York: H. V. & H. W. Poor.

Cassell's Family Magazine for September, The Magazine of Art for September, The Quiver for September. New York: Cassell & Co.

Beton Coignet and Goodridge System of Constructing and Repairing Railways, etc. Pamphlet.

The Application of Wire Rope Tramways for Purposes of Economical Transportation. By F. C. Roberts, C. E. Trenton: Trenton Iron Co.

MEASUREMENT OF THE FORCE OF GRAVITY AND MAGNETIC CONSTANTS AT BONIN ISLAND. By A. TANAKADATE, Tokio University.

SCHEDULE AND ITINERARY OF EXERCISES OF THE SCHOOL FOR ENGINEERS AT ROME, ITALY.

WATER METERS. By ROSS E. BROWNE. Science series, No. 81. New York: D. Van Nostrand.

This little book chiefly interests those people who have to employ or submit to the registration of their water supply by some one of the few mechanical devices which can presumably measure a flow of water of varying velocity.

Descriptions and accounts of comparative tests of the Worthington, the Kennedy, the Siemens, the Hesse, the Frazer and the Crown meters are given. The descriptions are rendered clear by excellent illustrations.

TRANSACTIONS OF THE AMERICAN INSTITUTE OF MINING ENGINEERS. Vol. XIII. New York: Published by the Institute.

This volume has been prepared for publication with remarkable celerity, the proceedings of the meeting of February last being included.

It comes as usual in the form of an exceedingly well-printed paper-bound volume; royal octavo size, 806 pp.

THE PRESERVATION OF TIMBER BY THE USE OF ANTISEPTICS. By SAMUEL BAGSTER BOULTON. Science series, No. 82. New York: D. Van Nostrand.

Engineers engaged in all branches of professional labor are interested in the subject of the preservation of timber.

The fact that underground conduits for telegraph lines are in some cases made of prepared timber lends a new interest to the subject. This little treatise presents the views of the members of the British Institution of Civil Engineers.

Many experiences are cited and valuable results recorded.

That the subject is held to be of prime importance is well proven by the fact that the last two issues of the Transactions of the American Society of Civil Engineers are devoted exclusively to this same subject.

A SYSTEM OF IRON RAILROAD BRIDGES FOR JAPAN. By J. A. L. WADDELL, C. E. Published by the University of Tokio.

This voluminous work, consisting of two volumes of royal octavo size, is designed to give instruction to engineering students in Japan. It affords, by aid of numerous plates and many tables, the means of designing iron bridges of the prominent American varieties, even to the smallest details.

The calculations are easy, and the descriptions of parts are drawn out in detail, to suit the condition of learners who are laboring with a newly-acquired language, and are seeking to solve new engineering problems.

The treatise is No. 11 of the Memoirs of the Tokio Daigaku, or University of Tokio. It is a credit to the accomplished author as an engineer and as an instructor.

A CATECHISM OF THE STEAM ENGINE. By JOHN BOURNE, C. E. New edition, much enlarged and mostly rewritten. New York: D. Appleton & Co.

No particular comment on the scope and aim of this book is called for. There is no extensive treatise on the steam-engine in the English language that does not quote or refer to Bourne's treatises, and nearly all students who have encountered the subject of steam, and have needed a clear and comprehensive elementary work, have been referred to Bourne's Catechism

A revision of the original well-known edition was naturally demanded by the recent progress in engineering. Such revision it has received at the hands of the author. The preface asserts that he has been unsparing in his excisions, but the total result of his revision is a larger book than before.

The part explanatory of the scientific principles of steam and gas engines is much fuller

than in the earlier edition, and the modern doctrine of thermodynamics is fully expounded. New forms of compound engines are described, and a new chapter on air and gas engines has been added. A collection of data for practical work is also a new and valuable addition.

MAIN DRAINAGE WORKS OF THE CITY OF BOSTON. By ELIOT C. CLARKE. Boston: Rockwell & Churchill.

This work will be gladly welcomed by the civil engineers throughout the country. The successful solution of a great engineering problem is described and profusely illustrated

The cure of defects in a system of sewerage, already established and long in use, involves intricate questions of health, economy, and public policy, apart from the engineering work. The particular solution for any city is dependent upon the location and topography In the case of Boston, the question of method was wisely submitted to a commission of eminent engineers. The history of the new system naturally begins with the report of this commission. An account of the survey follows, and then a description of the entire new system.

The work from 1876 to 1880 was under the direction of Joseph P. Davis, a prominent member of the American Society of Civil Engineers. Upon the resignation of Mr. Davis, Mr Henry M. Wightman became his successor. Mr. Clarke, the author of the present report, was the principal assistant-engineer in charge.

Boston is justly proud of her drainage, and American engineers may regard with satisfaction the evidence afforded by this work of the high order of achievement to be credited to the engineering profession in this country.

CHEMICAL PROBLEMS. By DR. KARL STAMMER, translated by W. S. HOCKINSON, A. M. Philadelphia: P. Blackiston, Son & Co.

Instruction in elementary chemistry is now considered incomplete unless it includes a course of problems. Of late it has been the custom with authors to append suitable examples to each section of their text-books. But many good text-books are deficient in this respect, and the need is then only supplied by separate books of problems. The neat little book before us will supply such a deficiency to a fair extent.

There are 101 problems in the book, involving the principal reactions in elementary chemistry, and with proper attention to their economic bearing. There is scarcely enough variety of kind, however. Problems relating to the determination of the symbols of compounds when the percentage composition is given; to determination of atomic or molecular weight from specific heat; to gaseous volumes of products in burning hydro-carbon vapors, and to electrolysis, might profitably be added to the otherwise good collection.

ELEMENTARY MECHANISM. By ALBERT W. STAHL, M. E., and ARTHUR F. WOODS. New York: D. Van Nostrand.

The attention of instructors in mechanism and of students of mechanical engineering is called to a new text-book on Elementary Mechanism, by Arthur F. Woods and Albert W. Stahl. The authors are engineer officers of the U. S. Navy, for several years past on duty as instructors in mechanical engineering.

The book was prepared by the authors on account of the difficulty of obtaining a suitable text-book for the use of their classes of students. There was no lack of works on the subject, but it seemed to the authors that, for their special purposes, some of the existing books were too vague and incomplete, while others partook more of the nature of exhaustive treatises than of text-books. The aim in the preparation of this volume has been to produce a clear, concise, and accurate *text-book*. All *purely* theoretical discussions have been avoided, except when they led to direct practical results. While much has thus been omitted that is of merely abstract interest, yet it is believed that nearly all that is of direct practical importance will be found in this volume.

An introductory chapter of definitions and general explanation is followed by a chapter on elementary propositions concerning the various modes of transmitting motion. The third chapter treats of the transmission of motion by rolling contact, and includes the discussion of rolling cylinders, cones, hyperboloids, ellipses, and multi-lobed wheels. The next four chapters treat of sliding contact. Special attention is given to the teeth of wheels, and full details are given of the practical method of laying out gearing of all kinds, both by the exact methods and by means of the different methods of approximation. Under the head of sliding contact are also shown the methods of laying out cams, screws, worm-wheels, slotted links, escapements, quick-return motions, etc. The next chapter is devoted to a full discussion of the transmission of motion by means of various kinds of linkwork. The usual click and ratchet motions come in this chapter. The following chapter treats of the transmission of motion by bands or belts, and includes the discussion of twisted belts, guide pulleys, speed cones, etc. Having thus discussed the four varieties of elementary combinations for the transmission of motion, the next chapter treats of the manner of employing them in trains of mechanism. The manner of designing practical trains of mechanism to give any desired velocity ratio is explained by a number of problems worked out in detail. The last chapter is devoted to the discussion of aggregate combinations, such as Weston's pulley, differential screw, parallel motions, drill-feed motions, oval chucks, etc.

A collection of practical problems is added, to give the student additional practice in the application of the principles acquired.

The book contains 300 pages, and is profusely illustrated.

EXTERIOR BALLISTICS. By CAPTAIN JAMES M. INGALLS, Captain First Artillery, Instructor Fort Monroe, Virginia. Printed at the United States Artillery School.

Captain Ingalls has just published the course of exterior ballistics in which he instructs the students of the Fort Monroe School. This course, remarkable for the spirit of method and of healthy discussion shown in its preparation,

is divided into two parts, which we shall pass in review successively.

I. *Resistance of the Air* —The author, remarking that the theory of gases is not sufficiently developed to permit a theoretical study of the question, announces that he is simply seeking an experimental law sufficiently approximate. He begins by establishing the law of the square of the velocity by the well-known demonstration based on the consideration of the impact of the moving plane against the gaseous molecules when the velocity is normal to its surface; then he treats the case of the oblique movement, and adopts the following formula, quoted by Poncelet in his *Méanique Industrielle:* $P=ps\frac{2}{1+sec^2\varepsilon}$. p being the pressure on the normal element, s the surface, and ε the angle which the plane element makes with the direction of the motion. He applies, then, this formula to surfaces of revolution as well as to projectiles with conical, ellipsoidal, and ogival heads, remarking that these applications have no experimental sanction, since, to have the true resistance, it would be necessary to eliminate the pressure on the rear portion of the projectile, a pressure whose value is unknown. We must, then, be content with empirical formulæ.

After an historical summary of former experiments, the author studies the method generally adopted to estimate the resistance of the air, knowing the velocity of the projectile in two points; he points out the error committed, the error of the principle, by admitting that in this intermediate path the resistance is constant, and corresponds to the mean of the two observed velocities; he calculates the error thus committed in the case in which the resistance would follow the law of the square or of the cube of the velocity. After having given a summary of the formulæ of General Mayevski, relative to spherical projectiles, he passes to Bashforth's method, based, as we know, on the measure of the time of passage of the projectile between ten consecutive and equidistant screens, and much more rigorous than the preceding method.

We know that Mr. Bashforth puts the resistance of the air under the form $2bv^3$, b being a constant, considered as constant in the analysis, but which varies, in reality, with the velocity, so that we must give it in each case a particular value taken from special tables.

Captain Ingalls, while adopting the results given by Mr. Bashforth, does not keep them under the form $2bv^3$, but in discussing the values of b, or, more correctly, of a constant k, which is proportional to it, he is led to write the following laws, viz.:

$$\rho=\frac{d^2}{g}\mathrm{A}\,f(v),$$

d being the diameter of the projectile in inches, g and v being expressed in feet; we shall have

For $v>1330'$, $f(v)=v^2$, log. A= $\bar{4}.1525284$
from $1330'$ to $1120'$, $f(v)=v^3$, log. A= $\bar{7}.0364351$
from $1120'$ to $990'$, $f(v)=v^6$, log. A=$\overline{17}.8865079$
from $990'$ to $790'$, $f(v)=v^3$, log. A= $\bar{8}.8754872$
from $790'$ to $100'$, $f(v)=v^2$, log. A= $\bar{5}.7703827$

These formulæ are analogous to those given by General Mayevski in 1882. Capt. Ingalls compares them, and remarks that these last give a lesser value for the resistance of the air, which he explains by the more elongated form of the Krupp projectiles. Finally, he closes this remarkable discussion by generalizing his formulæ, and extending them to projectiles of different forms by the aid of the following demonstration:

If we determine for the same velocity the values A and A′ of the coefficient A of the foregoing formulæ for the typical projectile and a different projectile, we can take, in general $\rho'=\frac{A'}{A}\rho$, ρ' being the resistance opposed to the second projectile, and ρ the resistance given by the above-mentioned formulæ, which allows our using for the new shell the ballistic tables calculated for the first.

This principle is admissible with the reservation, however, that we shall determine experimentally, for different velocities, the values of the ratio $\frac{A'}{A}$, in order to establish its constancy, or else the mean value to be used in each case.

II. *Motion of Projectiles.*—In this second part the author passes in review the different methods now employed; Bashforth's method, in which we express the different elements as functions of the angle θ of the tangent with the horizontal, and in which the data of the problem are changed by the adoption of a mean value for the variable coefficient "k" of the formula of resistance kv^3; Siacci's method, in which all the elements are given as functions of the velocity, and in which, as in Didion, the artifice consists in replacing $\frac{1}{\cos\theta}$ by a constant value a.

He applies this last method to the expressions that he has established to represent the resistance, and calculates the corresponding numerical tables.

It is to be noticed that the author has completely omitted the study of the "derivation," and has examined only the projection of the motion on the plane of fire. We do not know whether he reserves this question for another work, or whether, considering the lack of practical importance of the phenomenon, he has believed that he ought to neglect studying it—a study which the insufficiency of our knowledge of the theory of gases renders, moreover, somewhat unsubstantial.—*Translation—Revue d'Artillerie, May*, 1885

This is a high class of text-book. The calculations are in better shape, and the work is mastered and worked out more carefully than usual. The most noticeable original work is in the investigation of resistances to projectiles with heads of different shapes, although, of course, data will be found supplied by Mayevski and Bashforth. The writer appears to have studied most of the best works on the subject. Didion, Krupp, Siacci, Greenhill, Nivin and Mackinlay are quoted. The work is put in good shape by the author, and is a valuable contribution to the literature of gunnery. The

author points out how far English and foreign results are in accordance with each other, and discusses them fairly. It is important in a book of so technical a character to have the opinions of authorities on the subject, such as are quoted in the work itself. Three of these we know to be favorable.

Capt. Ingalls has received a letter from Madrid, requesting the privilege of translating his work for publication in the "Memorial de Artilleria."—*From The Engineer, London, July 24, 1885.*

MISCELLANEOUS.

A FLOATING dome was some time since presented by M. Bischoffsheim to the Observatory at Nice. It is intended to cover a colossal telescope; it is 22 m. in diameter in inside, and has a circumference of 60 m., or 2 m. more than the dome of the Pantheon. Instead of rendering it movable by placing it on rollers, according to the ordinary method, it is closed below by a reservoir for air, which rests on the water in a circular basin. This system of suspension is said to be so perfect that, in spite of its great weight, a single person can turn it completely round the horison.

ACCORDING to the report of Dr. Frankland, on water supplied to the metropolis during May, the Thames water sent out by the Chelsea, West Middlesex, Southwark, Grand Junction, and Lambeth Companies, exhibited a further improvement as regards organic matter, the average proportion being even less than in any month of last year. All the waters were clear and bright on delivery. Of the water drawn from the Lea, that distributed by the New River Company was, as regards organic matter, second only to the best of the deep-well waters; while the East London Company's supply contained rather more organic matter than the Thames waters. Both samples were clear and bright.

MARTIN KILIANA, of Munich, has recently described a new method of zinc extraction. The material to be worked—precipitated zinc oxide, calamine, calcined blende, zinc ashes, &c.—is placed in lead-lined wooden vats, and digested with a liquor consisting of ammonium and ammonium carbonate, until the liquor is saturated with zinc. After filtration, the solution then passes to a reservoir, from which it is fed continuously to the precipitation tanks. The cathodes in these are of zinc or brass, the anodes of sheet iron. A portion of the zinc in solution is precipitated in a compact metallic form on the cathodes, with a corresponding liberation of oxygen at the anodes. The liquor passes through the precipitation tanks at a speed regulated according to the amount of zinc contained and the strength of the current at the electrodes, and then flows into the reservoir at a lower level, from which it is pumped up again into the first tanks, to extract a fresh amount of zinc, and pass again to the precipitation tanks. All the vats and reservoirs are well covered over, to prevent loss of ammonia.

THE longest bicycle ride ever made has just been completed by Mr H. R. Goodwin, of the North Manchester Club. Leaving Land's End on June 1st, he journeyed to John O'Groats, having reached which point in 7½ days, he at once turned southward, and again arrived at Land's End on the 16th, the double journey of about 1750 miles, or from one extremity of England to the other, having occupied less than sixteen days. From Land's End he rode to London, which was reached on the 19th, the rider having just completed a journey of 2050 miles in exactly nineteen days, or an average of 108 miles per day. Mr. Goodwin rode a 40-inch "Facile" safety bicycle, and arrived in London well.

A SELECT Committee of the House of Lords, presided over by the Duke of Richmond, has passed the Southwark and Vauxhall Water Bill, which empowers that company to construct a reservoir at Forest Hill, in order to give a high-service pressure to Wimbledon. It also enables the company to affix stop-cocks to every service-pipe in their district for the purpose of preventing and detecting waste, which stop-cock must be paid for by the consumer. The bill also empowers the company to raise £250,000 additional capital, but the Committee have inserted a clause compelling the company to raise the sum by debenture stock to be issued by public tender at par. The clause by which the company sought powers to purchase the dust-sifting yard near their Battersea filter beds was struck out on the opposition of the Brighton Railway Company, the owners of the yard.

THE coal fields of Russia are, Mr. W. Mather says, still practically undeveloped. The Donetz coal field is too remote for the manufacturing districts, and the railroad communications are too uncertain to admit of its being largely used. The lignite found within a radius of 200 miles of Moscow does not offer fuel of a sufficiently good quality. It is a remarkable fact that during the past two years English coal has been found to be the most profitable fuel that manufacturers could use immediately around Moscow at a price laid down of about 40s. per ton. Twenty years ago the price of wood fuel was so low as to be equivalent to coal at 10s. per ton, and now coal at 40s. per ton is cheaper fuel. This is apparently a consequence of the reckless destruction of forests in Russia without systematic planting under Government supervision.

IN the *Bulletin de la Société d'Encouragement pour l'Industrie Nationale*, M. Carnot, in a report presented by him on behalf of the Committee of Chemical Arts, showed that the cause of the corrosion of sheet copper, employed for the sheathing of ships is the presence of cuprous oxide, which, in contact with salt water, occasioned the formation of soluble salts, even when the air is excluded. In order to reduce more completely the oxygen compounds present in the copper, he introduces a small quantity of metallic manganese, which completely reduces the cuprous oxide remaining in the metal, and becomes converted into a manganese silicate, in contact with the sides

and the sole of the furnace. If a few thousandths of manganese remain alloyed with the copper, they affect neither its malleability nor its resistance to the action of sea water. The manganese is introduced in the form of cupro-manganese, an alloy containing 75 per cent. of copper and 25 of manganese.

M. DE TROMELIN has attacked the hypothesis that clouds are composed of vesicles or hollow spheres of condensed vapor. He supposes that every solid body, whatever may be its diameter, retains around it by adhesion a special atmosphere of the gas in which it is plunged; that the thickness of this atmosphere is nearly independent of the volume of the solid body; that the attraction which retains it is within the domain of the molecular forces, and is manifested only within very short distances. In this way he accounts for the difficulty of completely removing the air from a tube which is to be filled with liquid. In the case of a vesicle surrounded by its atmosphere, the thermal absorption of the water is much greater than that of the surrounding diathermanous air. The atmosphere of the vesicle is consequently expanded, and the particle with its atmosphere floats by displacing an equal volume of the circumambient air. The dust particles which are observed in the sun's rays are supposed to be sustained in the same way.

THE report of Mr. William Crookes, F.R.S., Dr. William Odling, and Dr. C. Meymott Tidy on the water supplied to London during April, states that "throughout the preceding months of February and March, the water supply manifested, although but in a moderate degree, what may be called its wintry characteristics; which our results show to have been put off altogether in the supply of the past month. Thus, while the maximum proportion of organic carbon in the Thames-derived water furnished in February and March was .256 part, and the mean proportion .181 part in 100,000 parts of the water, the maximum proportion in the water furnished during the past month was .152 part, and the mean proportion .141 part in 100,000 parts of the water—the seasonal improvement, it is noticeable, being shown as much in the greater uniformity of the samples, as in the mean reduction of the rarely otherwise than small proportion of organic matter present. Of the 167 samples examined, the whole were found to be perfectly clear, bright, and well filtered."

IN his instructions relating to sewer works, Sir R. Rawlinson observes that Portland cement and lias limes make good hydraulic mortar. The proportions of cement, or of lime to sand, should not exceed 2½ of clean sharp sand to one by measure of ground Portland cement or lias lime. If clean furnace ashes or slag is available, there may be two of sand and one-half of ashes or slag, the whole to be mixed in a revolving pan, each panful to have twenty minutes' grinding. When mortar is used with bricks, the beds and joints should be spread thick and full over the entire area of both bed and joint, leaving, when pressed into place, a bed and joint never less than ¼ in. in thickness of mortar. In four cubic yards of completed brickwork there should not be less than one cubic yard of mortar incorporated. In making mortar or concrete it will be of the utmost importance to use clean materials and to preserve them clean. The water used for wetting bricks and for mixing concrete and mortar must be free from salt. Concrete and mortar should also be used on clean surfaces.

A BROKEN FILE.—There is no tool so easily broken as the file that the machinist has to work with, and is about the first thing that snaps when a kit of tools gets upset upon the cross-beam of a machine or a tool board from the bed of an engine lathe. It cannot even be passed from one workman to another without being broken, if the file is a new one or still good for anything, if an apprentice has got anything to do with it, and they are never worth mending, however great may be their first cost, unless the plaster of Paris and lime treatment can make a perfect weld without injuring the steel or disturbing the form of the teeth. Steel that is left as hard as a file is very brittle, and soft solder can hold as much on a steady pull if it has a new surface to work from. Take a file, as soon as it is broken, and wet the break with zinc dissolved in muriatic acid, and then tin over with the soldering iron. This must be done immediately as soon as the file is broken, as the break begins to oxydize when exposed to the air, and in an hour or two will gather sufficient to make it impossible for the parts to adhere. Heat the file as warm as it will bear without disturbing its temper as soon as well tinned, and press the two pieces firmly together, squeezing out nearly all the solder, and hold in place till the file cools. This can be done with very little to trim off, and every portion of the break fitting accurately in place. Bring both pieces in line with each other, and, for a file, it is as strong in one place as in another, and is all that could be asked for under the very best of welding treatment.—*Boston Journal of Commerce.*

THE incandescent lamp life-test which has been going on at the Franklin Institute, Philadelphia, has reached its 1064th hour. The *Scientific American* says the Edison, the Weston, the Stanley, and the Woodhouse and Rawson companies competed. The Sawyer-Man and Brush-Swan companies were invited, but declined to participate in the trial. Extraordinary precautions were taken to prevent access to the lamps except by members of the committee. The lamps were lighted on April 11th, and have burned ever since. At 11.35 this morning the Edison Company, who had entered 21 lamps, had lost 1; the United States Company, who entered 24 had lost 17; the Stanley Company had lost 19 out of 22, and Woodhouse and Rawson, an English firm, had lost 11, or their whole number entered. The Edison Company used the natural fiber bamboo carbon, while the Weston people used the artificial tamidene carbon.